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LECTURES
ON
COMPARATIVE ANATOMY
AND
ANIMAL PHYSIOLOGY,

DELIVERED BY

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LECTURE XLIV.

ON THE DIGESTIVE ORGANS OF FISHES, AMPHIBIA, AND REPTILES.

Of all the organs of nutrition, or of vegetative life, the digestive are those which are most immediately related to the kind of food, the peculiar instincts and habits, and the whole structure and internal economy, of animals. We have seen, throughout the invertebrated tribes, that the form and the development of their digestive apparatus are most intimately and necessarily connected with their means of perceiving, distinguishing, and obtaining their food, their organs of perception and locomotion; so that the organs of the senses, the nervous system, and even the muscular and the osseous systems, have their structure, and forms, and extent of development, in perfect accordance with the condition of the digestive organs. Indeed the organs of *animal life*, notwithstanding their importance and their high physiological character, are subordinate to those more general organs of individual nutrition or of *vegetative life*, and are developed in accordance with their condition. The monad is ciliated to move about, and select and seize its prey, because it has many hungry stomachs to satisfy with particular kinds of food; but the poriferous animal, feeding on the simplest forms of organic matter diffused through the mass of the ocean, needs no such power of locomotion, and it grows, fixed like a lichen, on the rocks. When the digestive apparatus becomes more exquisitely organized, and adjusted for the assimilation of matter only in a particular condition, it must necessarily be provided with numerous organs of sense, and organs of locomotion and prehension, to distinguish and obtain this peculiar form of matter. These mutual relations necessarily exist between all the parts of the same animated machine, and their study leads to the discovery of those interesting laws which regulate the coexistence of organs and parts in animal bodies. It is by an acquaintance with these laws that we are enabled, from the structure of one organ, to infer that of many others, and from the examination of a part of an animal to construct its whole fabric.

In the class of fishes we see the first and lowest vertebral condition of the alimentary canal, and of the whole digestive apparatus. Fishes, voracious to a proverb, subsist almost entirely on animal food. The ocean teams chiefly with animal life. It is a dense and rich and moving and tempestuous element, where vegetation is comparatively small, contrasted with its development in the light and unresisting element of the atmosphere. This rich and resisting element of water abounds, in every latitude and in every drop, with all forms of animated beings. We observe, thus, that fishes have the means of easily satisfying their voracious appetites with a selection of all kinds of food in the animals that team through every latitude, and through every stratum of the ocean which encompasses so large a portion of the globe. Their teeth are not generally instruments for mastication, but, as we have seen, are formed for prehension, and are often fixed, with a solid osseous connexion, to the gums, by the ossification of their pulp, without roots or alveoli, not opposed to each other like flat-crowned teeth;

they are sharp, recurved, similar, dense and pointed cones; they are often loosely attached to the jaws, disposed in numerous rows, consisting chiefly of bone, with little or no enamel, and therefore it is that we find their central pulp so frequently ossified and anchylosed to the surface of the jaws in more advanced life. Those crowns of teeth are obviously mere prehensile organs, as you observe frequently developed in different parts of the mouth in other classes, as on the tongue in birds, on the vomer of amphibia, on the palatine bones of serpents, on the tongue of molluscous animals, and even on the tongue of fishes. Those prehensile organs, adapted to grasp and retain every living thing that moves in the waters, are placed in all parts of the mouth of these all-devouring animals; they are placed often on the branchial arches, on the os hyoides, on the intermaxillary bones, on the palatine bones, on the vomer, on the pharyngeal bones, as well as on the upper and lower jaw-bones. Some fishes are entirely destitute of teeth of every kind. The tongue is generally very short, broad, and fleshy, in fishes; their œsophagus is generally very wide and short, and with internal folds of the mucous coat, and opens directly into their capacious stomach. Thus the food of fishes not being masticated in the mouth does not dwell there, and as there is abundance of moisture with their food, they require no salivary glands, and have none. Their œsophagus is wide, because their food is swallowed entire.

Now then fishes, which are but hungry larvæ, have thus their large stomach almost opening on their outer surface, so wide and short generally is the œsophagus. They are all stomach, like the larvæ of other classes, and are chiefly intent upon the gratification of their voracious appetites. All other sense seems to be absorbed in this. No soft sounds to charm their ears are heard in the silent deep. No roaring of lions, or other fierce animals, to excite their alarm. They are dumb, because they have no lungs nor larynx; and so they should be, as they are so deaf. Their huge eyeballs, what can they see to engage their attention but their prey in the vast dark deep, clouded with mud, or the ever-moving sands? You see, therefore, that all their senses but that of hunger are obtuse. Look at their small brain; it is permanently in the very embryo state, the very earliest condition of the human fœtal brain. And thus fishes have little to mind but their stomach, which is a crucible adapted for the decomposition of all kinds of organized matter. The stomach of fishes is often in the form of a large spherical sac, with its cardiac orifice near to its pyloric, as you see in this *lophius*, and with strong muscular parietes. We observe the œsophagus often very thick and muscular, and marked by longitudinal or transverse folds of the mucous coat, which extend along the cavity of the stomach, and the beginning of the stomach is often indistinguishable from the œsophagus. Sometimes this stomach is more of a flask form, dilating gradually from its commencement at the cardiac orifice to the middle, then tapering downwards to its lower closed extremity, as you see in the *polypterus*, the *xiphias*, and many other fishes.

The pyloric orifice is variously situate upon the polymorphous stomach. Sometimes it is placed near to the cardiac orifice, as you see in the *sword-fish*, and the *lophius*; sometimes, as in the *tetraodon* and the *lamprey*, we observe it placed at the lower extremity of the stomach; sometimes the stomach bends upon itself, as we see in the rays and the sharks; this is the common form in the higher cartilaginous fishes. The stomach here, in the shark, passes down wide and then forms an angle, turning upwards, and becoming narrower and more muscular towards the pyloric extremity. The pyloric muscular part of the stomach in fishes is often strong as a gizzard. At this pyloric extremity of the stomach we observe the mucous coat, passing freely inwards, and forming a circular projecting fold of the mucous coat, with rough margin, extending into the interior; this is the pyloric valve, which is remarkable for its constancy in this class. In the molluscous animals similar valvular folds are common. In the class of fishes, beyond this pyloric valve we observe the orifices of the hepatic and the pancreatic glands.

Those pancreatic glands in the osseous fishes have the form of small *appendicula cæca*, more or less numerous, that open around the pyloric extremity of the stomach immediately beyond the pyloric valve, pouring their secretion, of a turbid white appearance and thick consistence, into the commencement of the duodenum. This is the form and position of the pancreas in the osseous fishes; but those pyloric cæca vary very much in their number and forms. Sometimes there are hundreds of them communicating with each other, and collected together in this situation around the pyloric end of the stomach, as in the mackerel; or opening by one great orifice at the commencement of the duodenum, as in the *sturgeon* and in the *sword-fish*. We find the number diminishing until we are left, in the *perch*, with only three cæca, and in the *lophius* and the *pleu-*

ronectes with only two. The nature and relations of those appendices, however, are not in the least altered by there being only two, or as many as two hundred. In the cartilaginous fishes the pancreas has a conglomerate appearance, from these cæca being so much lengthened and so much subdivided.

In the digestive organs of the *xiphias gladius* (*Fig.* 160), which is a powerful predaceous fish inhabiting the open seas, we observe a very strong muscular œsophagus leading to a closed, lengthened, bottle-shaped stomach (*Fig.* 160, *a*), with very thick parietes and a strong cardiac sphincter. The pyloric orifice (*Fig.* 160, *b*) is near the cardiac, at the anterior extremity of this long narrow sac, and has a very distinct internal fold of the mucous coat forming a circular valve. Immediately beyond this valve is seen the entrance of the *ductus communis choledochus*, and the wide opening of the general duct (*Fig.* 160, *f*) of all the pancreatic cæca, which here form a large reniform mass (*Fig.* 160, *e*), covered with a muscular tunic and the peritoneum. The liver (*Fig.* 160, *c*) is undivided, rather small, and sends out three long hepatic ducts, which enter the cystic near to its termination in the duodenum. The gall-bladder (*Fig.* 160, *d*) is curved, pyriform, with a long cystic duct. The small intestine (*Fig.* 160, *b*, *g*) is long, narrow, and muscular, and presents an internal circular fold of the mucous coat projecting free where it terminates in the very short, wide, and straight colon (*Fig.* 160, *h*), forming a distinct valvula coli. The pancreas (*Fig.* 161) of this animal when opened shows its innumerable component cæca connected together into a large kidney-shaped mass by means of intervening cellular tissue and capillary vessels. This large pancreas is composed of innumerable shut sacs, each about a line in diameter, the closed ends of which are seen all over the surface of the organ. These little cæca are filled with a thick white secretion, and are formed by the successive divisions of the single great duct by which they all pour their contents into the duodenum immediately below the pyloric valve, as I described many years ago in the *Medico-Chirurgical Transactions* of Edinburgh.

Fig. 160

Each cæcum composing the pancreas of the *sturgeon* does not open separately into the duodenum as stated by Cuvier, but by one great orifice, as shown by Monro, and as I have shown also in the *xiphias*. If we are reduced in the class of fishes to this very simple form of the pancreas, consisting of two cæca, obviously traced to be this organ, by following it from man downwards, or from this form up to man's, in what form should you expect to find this organ when you looked for it lower down in the scale of animals? Somewhat, surely, of the same simple structure as we saw it in the gasteropods, in the condition of a single pyloric cæcum, conjectured rightly by Carus to be the analogue of this organ.

It is interesting to trace downwards the forms of those well-known glands, and to seek thus for their simplest forms in the vertebrata, in order that we may recognise the still simpler forms of the same organs when they present themselves among the invertebrated classes.

In the cartilaginous fishes the pancreas assumes the conglomerate form which it presents in the higher forms of vertebrata. In the sword-fish, that organ is very large; it is larger than the liver itself; it weighs six ounces more than the liver; it consists in the sturgeon, as in the xiphias, of numerous cæca enclosed in one mass in a muscular sheath, hundreds of cæca that are all ramified from one great duct or orifice, and that orifice opens into the duodenum close to the anal side of the pyloric valve, and to the termination of the *ductus communis choledochus*. This complex pancreas is surrounded externally by a peritoneal and a muscular coat. This muscular structure of the exterior tunic compresses that large glandular organ, and forces out the secretion which its numerous component cæca contain. In the cartilaginous fishes there is a true conglomerous gland, the most highly developed form.

The intestine of fishes varies considerably in its length, according to the kind of food; it is for the most part short, and sometimes is much shorter than the length of the body. In the flying-fish you will observe that it passes in a straight line from the mouth to the anus, and is thus not half the length of the body, and yet this is in a vertebrated animal. It is

almost equally short and straight in the lamprey. At other times it has numerous and long convolutions. At a short distance before it terminates in the rectum, we observe that it sends inwards a fold of the mucous coat, which forms a valvular projection into the interior of the cavity of the intestine; that is the valve of the colon; the colon from that part is generally somewhat, though very little, enlarged to the extremity of the rectum. In the river lamprey, *petromizon fluviatilis*, before you, you observe the œsophagus, stomach, and intestine, passing nearly in a straight line from the mouth to the anus, the whole alimentary canal being shorter than the length of the body. In the cartilaginous fishes we observe, in the rays and sharks, that on the exterior the intestine appears remarkably short, and almost straight from the pyloric extremity of the stomach to the anus, but the food has a very extensive course to pass through in going through this apparently straight intestine, in consequence of a convoluted fold that is seen to wind round in a spiral manner through its whole interior. This convoluted fold is seen also in the sturgeon and most chondropterygii. In the rays it presents an extensive surface, from the close approximation of those convolutions; but in the sharks, the spiral turns of this mucous fold are placed at a greater distance from each other, although the kind of structure is essentially the same as in the rays, formed like the turns of a stair, along the whole course of which the food has to pass. We observe the intestine of considerable length in several of those fishes, although their food is not vegetable. This is frequently on account of the low degree of organization of the animals upon which these fishes chiefly subsist, and, consequently, the degree of elaboration which such food has to undergo before it can be brought to the condition of their body. There is no cæcum on the colon, but only the valvula coli. The alimentary canal in fishes passes smooth, without valvulæ conniventes, but with villi, and often rugæ, and terminates in a short general cloaca, which receives the terminations of the two oviducts in the female, as well as the terminations of the urinary organs, and in the male the terminations of the two spermatic vessels. They are differently placed here, however, from the position which they occupy with relation to each other in quadrupeds; for if you consider their position in this specimen of the father-lasher, you observe that on the anterior part is the opening of the intestine; the rectum is seen passing down in front of the ovaries. The middle part presents the single opening of the two united oviducts. The oviducts, which in osseous fishes are only the anterior portions of the ovaries, communicate with each other before their final opening, and form thus, apparently, one ovary or oviduct, with its upper part divided into two. The posterior part of this general vent, that next to the tail, presents the opening of the urinary organs, sometimes with and sometimes without a bladder. In the mammalia the rectum is placed behind the urinary organs in front, and the genital organs in the middle. On each side of the anus in the cartilaginous fishes, there is a valvular oblique opening which leads into the cavity of the peritoneum, as you observe in all the rays and sharks. These two peritoneal openings at the sides of the anus are seen in other fishes, as the lamprey, the sturgeon, the sword-fish, the salmon, and also in animals of aquatic habits belonging to the class of reptiles, as we see in the gavials, the crocodiles, and the alligators, and may serve to allow some peritoneal fluid secretion to escape, though their valvular oblique orifice will permit nothing to enter. The anus in fishes is never at the end of the trunk, and is generally considerably anterior to the end even of the abdominal cavity, which often extends far backwards into what appears to be the caudal part of the trunk.

Now as this digestive apparatus of fishes is its simplest form met with in the vertebrata, we find it to present all that diversity which we commonly observe in the lowest conditions of organs. The distinction of great and small intestine is sometimes obvious, and sometimes quite imperceptible. Although there are yet no valvulæ conniventes, the mucous surface is often greatly extended by longitudinal or transverse rugæ or plicæ, or by its villous surface. The alimentary canal is short in fishes, because they are yet low in the scale, and their food has less change to undergo to bring it to their condition; but their short canal possesses the power of quick and effective digestion, by means of the strong muscular coat of the stomach and intestine; and by the great development of the liver, and of the pancreas, and other glandular cryptæ opening into its interior. Swallowing everything they meet, they can reject from their stomach the shells and other indigestible parts of their food, like rapacious birds. Their liver is always large, and generally light-coloured and oily, but their pancreas we have seen to present every stage of development, from two simple cæca to its most conglomerate form, and often it is entirely wanting. The rudiments of salivary glands are seen in the numerous

small glandular cryptæ, which often give a spongy or cellular appearance to their palate, as shown by RATHKE in the *carp*. The spleen, sometimes single and sometimes divided, is found attached to the stomach or to the intestine, and the mesentery is now a constant part, to give the intestine attachment to the vertebral column, and to support the ramifications of bloodvessels, lacteals, and nerves. The air-bag generally communicates with the intestine, the stomach, or the œsophagus, by the ductus pneumaticus, but does not yet serve as an organ of respiration. The cavity of the abdomen is here separated from that which contains the heart and the respiratory apparatus, by a diaphragm still entirely membranous, and its peritoneal serous cavity opens, often externally, by the two lateral anal passages.

The tadpoles of amphibia are all stomach and voracity, like the fishes—the great tadpoles of the sea, and like the voracious larvæ of many insects; but by the metamorphosis of the tadpole, as by the change of the larva to the perfect insect, the long and capacious intestine is greatly reduced in extent, and adapted to the change of food, and the new condition of all the other organs of the body. The jaws and muscular apparatus for mastication are still feeble in the amphibia, and the food is mostly swallowed entire. The teeth are mere prehensile, slender, conical, sharp spines, placed sometimes, as in fishes, in numerous rows on the palate and jaws, as in the siren, or, more serpent-like, in single rows on the palatine bones and on both jaws, as in the common tritons, or only on the upper jaw and the palatine bones, as in the frogs; but the toad and the pipa have both jaws destitute of teeth. The long, free, and cleft tongue of the frog approaches much more to the ordinary form of the bifurcated tongue of higher reptiles than the short, thick, fleshy form of that organ in the perenni-branchiate species of amphibia. The strong muscular and dilatable œsophagus leads to a lengthened narrow stomach, directed transversely from left to right, and generally with thick fleshy parietes, covered above by the two lobes of the liver, and having a single lengthened spleen attached to its left side. The stomach is most lengthened and narrow in the tadpole and the aquatic species, and these exhibit the least distinction between the small intestine and the colon. The young tadpole of the common frog (*Fig.* 161), with its fish-like form and its small round sucking mouth, feeds on all kinds of soft animal and vegetable matter found in the fresh-water ponds and stagnant pools, and possesses an alimentary canal of extraordinary length adapted to this simple and mixed kind of food. The intestine is nearly equal throughout and only a little enlarged towards the end of the rectum, so that there

Fig. 161

is still as little distinction of great and small intestine as in the fishes, and this equality of the alimentary tube is its embryo condition in all higher animals. The long intestine is easily seen through the transparent abdominal parietes of the young tadpole, coiled in a circular manner upon itself, and distending the abdomen by its numerous convolutions. Proportioned to the trunk of the young tadpole, the intestine has at this period many times the length which it has in the adult animal, when the food consists entirely of prey of a higher character, as snails, worms, caterpillars, and similar animals found creeping on the ground or on plants. During the metamorphosis there is no part of the economy of these most mutable animals that undergoes greater changes than the intestine and the whole alimentary apparatus. Those changes we have already traced in the osseous system, especially in the os hyoides and the vertebral column, and in the nervous system, as well as in the disposition of their muscular apparatus connected with respiration and locomotion. In the intermediate stages of the metamorphosis we observe the different conditions of their alimentary cavity, it becomes gradually shorter, as the food changes, and in the adult state of the

frog (*Fig.* 162) it is not one-quarter of the length, proportioned to the magnitude of the body, which it had in the tadpole state, when its numerous long convolutions distended the abdominal cavity.

Fig. 162

The stomach presents a thick muscular and pyriform sac in the adult of the common frog (*Fig.* 162, *m n*) and in the toad; and it is still of a lengthened form, as in most of the amphibious animals, as you observe in this land *salamander*, and in this common aquatic crested *triton*, where you observe it covered entirely, above and before, by the two lobes of the liver, and having the spleen fixed by cellular substance and vessels to its left side. The intestine performs a few convolutions of inconsiderable width in the adult state of the frog, and is marked in the duodenum by several transverse folds of the mucous coat. It presents a little difference between its small and its large intestine, which is wider and more straight in its course; and it terminates in a general cloaca, which also receives, as in the fishes and other oviparous vertebrata, the terminations of the urinary and genital organs. At the back part of the lengthened stomach we almost always find a long narrow pancreas, which partakes of the lengthened form of the liver and spleen and stomach. These viscera have generally a more lengthened form in the tailed amphibia than in those which lose that organ of motion. You observe that same lengthened and undivided form of the liver, of the pancreas, of the spleen, and of the whole alimentary canal, in this *axolotl* of Mexico, and in other aquatic species. Thus we observe that, in the different constitutions of these changeable amphibious animals, the intestine and the whole alimentary apparatus undergo changes to adapt them to the different kinds of food on which they subsist in the different stages of their metamorphosis. Feeding at first on the minutest animals and the softest aquatic plants, of which there is a never-failing abundance in the rich aquatic medium in which they are at first developed, we observe they are at length able by the development of their legs and their lungs to come to the banks of the pools of water they inhabit, and gradually to seize larvæ, snails, worms, and similar nutritious food, until at last their aquatic, fish-like career terminates in an almost terrestrial reptile condition. In this last stage they are confined thus to animal food of a higher character. So that the alimentary canal in those which undergo a metamorphosis is twice changed in its essential condition. It begins in the very young tadpole by being very short and small, and equal throughout, as in all other vertebrata in the early embryo state; it then rapidly assumes the long convoluted form of intestine, where the stomach is but imperfectly marked in the more mature tadpole; and at length it again becomes shortened in the adult frog, when all the divisions of the intestine become more distinct and obvious.

The serpents still present teeth on the palatine and intermaxillary bones, as well as in both jaws, but like the teeth of inferior vertebrata, they are still nearly simple crowns, sharp, conical, incurvated, and are adapted for prehension, not for mastication. Neither their loose jaws nor their slender sharp unopposed teeth are adapted for this function. Like the worms and larvæ they consist of the mere trunk of higher animals, and all their internal organs partake of the long cylindrical form of their body. The salivary glands vary in the extent of their development, as we see also the accompanying destructive poison gland. They feed on living prey, and their scaly lips and long filiform bifurcated tongue, employed as an organ of touch, are little adapted to give acute feelings from their food, which passes through their mouth and œsophagus undivided. The œsophagus is therefore capable of enormous distention, and in its collapsed state presents a longitudinally plicated appearance, with thin and very elastic parietes, lubricated by a most

copious secretion of mucus. This highly dilatable character of the first portion of the alimentary canal, for the swallowing of entire prey, corresponds with their want of prehensile hands or feet, and accords with the condition of the bones around the mouth, so that they are able to convey through the mouth and the œsophagus animals many times the diameter of their own body, as you observe in the dissected specimen before you, where a snake with the neck smaller than the point of the little finger has yet conveyed into its stomach without subdivision a large full-grown frog ten times that size. The œsophagus thus passes insensibly into the stomach as one wide lengthened pouch. The stomach is thus capable of being dilated to many times the diameter of the natural condition of the trunk, as you observe in the stomachs of the boa constrictors before you. The œsophagus, the stomach, and the whole external parts of the trunk of the body, we observe to possess alike the power of dilatation which we already saw in the jaws. The exterior integuments are highly elastic, and are covered not by large plates of horny substance, as in the crocodiles, or with fixed bones and large plates of horn, as in the chelonian reptiles, but with small detached pieces, that can easily be separated to a distance from each other, and allow the trunk to expand. The mucous coat here is remarkably thin and villous, and presents an immense surface for the secretion of mucus. At the lower or pyloric extremity of this longitudinally extended stomach we observe a contraction where the muscular coat is thickest, and the sphincter muscle and valve of the pylorus are placed. From this point of the commencement of the duodenum, the intestine, in this *boa*, presents numerous very small convolutions for about a sixth part of its length. The whole of the small intestine is here convoluted into a very small space; these convolutions are fixed to each other by cellular substance and by numerous bloodvessels. You require to cut this cellular connecting tissue in order to separate those convolutions from each other, and to perceive the length of this small intestine. At first view, on opening the boa constrictor or any of these serpents, this intestine appears twisted, as if it were the spiral intestine of a ray or of a shark, with a spiral fold passing into the interior; it has that appearance until you have dissected its turns asunder, and shown the simple convolutions of this small intestine. Where the small intestine terminates in the large, there is a small cæcum of the colon; that cæcum has a tapering form, and in a boa constrictor of ten feet long the cæcum of the colon is between three and four inches in length, and about an inch and a half in breadth; commencing wide, from the beginning of the colon it tapers up to its shut extremity. It is bound to the small intestine likewise by cellular tissue and by the peritoneum; and if you did not dissect out this cæcum of the colon, you would not ascertain that it had such a cæcum at all; indeed you will generally find it stated that reptiles are destitute of a cæcum of the colon, but here I have dissected it out and left it exposed to view. The colon proceeds wide and straight to the termination in the cloaca, which receives also the terminations of the two oviducts in the female, the two ureters, and, in the male, also the two grooved organs of intermission when double, as well as the terminations of the two spermatic vessels which end at the base of the grooved male organs. The liver and the other glandular organs of the abdomen, as the spleen, the pancreas, the kidneys, and the testes or ovaria, present the same lengthened form which we observe in the stomach and in the other parts of the alimentary cavity in the ophidian reptiles. The pancreas in the serpents is divided into numerous lobes, and those lobes send out each a separate duct which continues free into the duodenum. That structure has lately been described by my friend Dr. Duvernoy. It appears to me to point out a beautiful transition from that simple condition of the detached pyloric cæca in fishes where those little glandular portions of the pancreas open separately into the intestine, to the still more concentrated form which we meet with in birds, where there are still separate lobes and pancreatic ducts, and thence to the mammiferous form, where there is one gland and one duct. In the lowest reptiles, as the *cæcilia*, we still find the intestine, as in the lowest fishes, passing straight through the body, and shorter than the trunk, and we observed the same short course followed by the alimentary canal in the carnivorous serpentine forms of the articulated classes.

The crocodiles and the lizard tribes are mostly carnivorous, like the serpents, and feed generally on entire and living prey; so that we still find in them the short membranous and simple alimentary canal. Their digestive apparatus is for the most part short and capacious, particularly in its first or anterior part. In most of these lizards, we perceive the stomach to be a comparatively small sac, with its apertures placed at the opposite extremities, having seldom any appearance of a fundus or cæcum, or any means of retarding the passage of food through that cavity. In

these loricated crocodilian animals, however, we observe a stricture of the stomach, rather remarkable. The stomach there is of a globular form, and is provided with thick, strong, muscular parietes, constituting a true gizzard. The strong muscular parietes of this gizzard of the crocodile you observe in the specimen before you, the crocodile of the Nile, in the central shining silvery tendon on each side of the stomach, to which the lateral radiating muscles are attached. Now these animals are observed to have in their stomach frequently along with their food, stones and gravel, as if to assist in its subdivision. We still find in the sauria, traces of the pyloric and colic valves, so large in fishes. There is but little difference between great and small intestines; the large intestines do not yet present that puckered wide appearance which we observe in the large intestine or colon of the mammalia. Some of the saurian reptiles feed on vegetable food, though comparatively few. In those, as in the *iguana* and the *scinck* before you, the intestine is long and convoluted and capacious. The whole habits and dispositions of these animals correspond with their kind of food. The flesh of the yguana is very different in its quality from that of the saurian reptiles that feed on animal food. The flesh of the large crocodilian reptiles is tough and rancid, and is not eaten, but the palatable flesh of the *iguana* is prized as an article of food in South America, where the animal abounds. We observe this also with regard to the flesh of the scinck, which feeds on vegetable substances, an animal well known to the Arabians, and the flesh of which was supposed to be an antidote to many diseases. But most sauria are carnivorous, and hence the constancy of their gall-bladder, as in the carnivora of other classes. The intestines vary thus with their kind of food, like the stomach. The tongue is often long and filiform, and cleft, as in the monitors and lizards, or short and fleshy, as in the crocodilian sauria; it is a very long projectile slender organ in the chameleon, with a broad clavate termination for seizing flies. The cæcum coli, though generally present, is very small and short, and the colon is furnished with a valve. The liver is always provided with a gall-bladder, the spleen has a lengthened form, and the pancreas has generally a more compact form than in the serpents. The cæcum of the colon is wanting in the crocodile and some of the monitors, which, notwithstanding, possess the ordinary valvula coli, like most of the other sauria which have a small cæcum.

In the *chelonian* reptiles we observe animals that, for the most part, whether they live upon the land, or inhabit the water, feed upon vegetable substances. They possess salivary glands of various degrees of development, and in some they have not been detected. Those that inhabit the deep, or the great turtles that are so much sought after as an article of food, and the shells of which—the horny coverings of the ribs—are so highly prized in commerce for the uses that are made of them in the arts—they feed chiefly on marine plants; and we have seen the sharp horny bills by which they were enabled to divide and to bruise those succulent marine plants which render their flesh so delicate, and colour it green. We find, in the land-tortoises, likewise, which feed upon vegetable food, that the intestine is comparatively wide, long, and convoluted; that the tongue is short, fleshy, and villous; the œsophagus is wide, with longitudinal plicæ, and of considerable length, on account of the great length of the neck.

The œsophagus of the chelonia is strong and muscular, and is marked with longitudinal plicæ, folds of the mucous coat, which extend into its inner surface. Those longitudinal plicæ of the thick muscular œsophagus in the chelonia we observe to pass along a considerable portion of the stomach, along the whole of the membranous or cardiac half, and to be even slightly perceptible upon the strong muscular pyloric extremity of the stomach, which is so remarkable for its strength and thickness in this part in the chelonian reptiles. Those animals have always the tongue short and fleshy, and covered with numerous long papillæ directed backwards, and presenting a villous surface. The stomach lies transversely, below the two lobes of the large liver. The intestine you observe in those chelonian reptiles to be of considerable length and convoluted, occupying a considerable portion of the large and broad trunk of the body. The small intestine, like the œsophagus and stomach, is marked by longitudinal rugæ and folds of the mucous coat.

We have here the digestive apparatus contained in the abdominal or ventral portion of the great cavity of the trunk, which is occupied upon its dorsal aspect by the large and capacious lungs, extending backwards to the pelvis itself, and separated by the peritoneum from the cavity containing the digestive viscera. The position of the stomach, with the two large lobes of the broad expanded liver hanging over it, and the spleen on its left side, are seen in the preparations before you. The gall-bladder, which is always present, enters the duodenum very near to the pylorus, and be-

sides its duct there is a distinct hepatic duct which receives that of the pancreas, and opens into the duodenum. We observe at the termination of the small intestine in the wide colon, that it enters in such an oblique manner as to leave a pretty considerable blind enlargement or cæcum at the commencement of the colon. The parietes of the alimentary canal are strong and muscular throughout. The mucous coat is of great extent, as in other phytophagous animals; and although there are yet no valvulæ conniventes, this inner tunic forms numerous longitudinal folds and cells in the course of the intestine, by which its secreting surface, and the surface for the distribution of the numerous larger lacteal vessels, are greatly extended. The distinction of the small and large intestine is more marked here than in the inferior reptiles, amphibia, or fishes, and the long and capacious colon of the *chelonia* is provided with a distinct valve, and often with a wide and short cæcum, so that the alimentary canal has now acquired nearly all those characters and divisions which it presents in higher stages of development in the warm-blooded vertebrata.

ANATOMY AND PHYSIOLOGY.

LECTURES

ILLUSTRATED BY THE

HUNTERIAN PREPARATIONS

IN THE MUSEUM OF THE ROYAL COLLEGE OF SURGEONS, LONDON.

Delivered at the College in 1833, *by*

SIR CHARLES BELL, K.G.H., F.R.S.

LECTURE X.

RELATIONS EXISTING BETWEEN THE NERVOUS AND MUSCULAR SYSTEMS, AND MENTAL AND BODILY EXPRESSION.

GENTLEMEN,—Hitherto you know I have taken sure ground; I have gone as far as it was possible for me to be directed by the preparations and the arrangements of Mr. HUNTER. But now I must either go on alone, and unsupported, or I must move in another circle.

Now I believe that you would rather that I prosecuted the subject of muscular action a little further. May I observe, then, that the muscular system may be considered as that part of the organization of the body which is under the direction of the will by volition, and is used in locomotion; that, again, the muscular system is used in performing all those secret functions which are going on without the cognizance of the mind, and all the secret operations which are necessary to the performances of the animal economy.

Now I wish to draw your attention to this,—that the muscular texture which is so directly under the influence of the mind and passions, which is expressive of the condition of the mind, not by volition, but made evident to the eye by certain symptoms, is brought into action by certain nerves which are intermediate between the internal parts of the body and their external or muscular frame. This may appear to some foreign to our proper subject. Nothing, however, can be foreign on this occasion, which is a fair deduction from the facts presented to us by anatomy, a science which is highly cultivated only when we observe *all* its relations. If you find an interest in attending even to the number of bones in the wrist and the foot; nay more, if you find that you form as it were another science by an attention to the minutiæ of animals, you will not consider that time to be lost which is spent in analysing the relation established between the corporeal frame and the mind itself, the more especially as this is, in fact, laying the foundation of the doctrine of sympathy, or the doctrine of symptoms, as I should more correctly express myself. For how are we to be alive to all the changes in the body, and all the variations of form in disease, unless we are familiar with the condition and relation which are established between the nerves and emotions of the body?

I will give you a familiar instance in illustration of my view. I had bought a horse, and driven it to the door of a patient, who was looking out of the window, and who, on seeing me, said, "How long have you had that horse?" I told him that I had bought it a fortnight since. Then, said he, "you have made a bad bargain in it, for he is not sound." "How can you presume," said I, "to say so, for you have only looked at him from a distance?" "True," he replied, and I pressed him to tell me how he knew the state of the animal. "By its breathing," he said; "by the inflation of the nostrils." I was then satisfied that he was wrong in his opinion, for I found that I had not driven him in his own collar, and that he was only restrained in his respiration, and not unsound in his lungs. But I was well pleased to find that this knowing gentleman judged of the condition of the lungs by the action of the nostril. I have seen the same kind of thing in the hospital.

Going into the hospital, and finding nothing amiss, nothing to do amongst the patients, I have inquired if they were all going on well, and have been told that everything was right. But, on looking to some one man, I have found that although he was almost totally enveloped in bedclothes, yet that there was an unusual expression observable in his nostrils, and on looking to him more narrowly, I found a trembling of the lips. I have then said to myself, "Why, this man cannot be well; this is not the condition of a person in repose; he has nothing to alarm his mind, therefore this must be an indication or a symptom of something wrong." If you go up to the man on such an occasion, and ask him how he is, you find that the voice is affected,—that he cannot speak to you. If you make him draw in his breath, you find at once where is the seat of complaint; you put your finger to the place, and discover that condition in which the lungs are going rapidly into a state of hepatization, in which they are fast becoming to resemble liver. Now I certainly believe that the course of reflection we are upon to-day, although it may take us, apparently, a circuitous route, will lead the mind to be more alive to little circumstances which would otherwise escape observation.

We find that some men are observers, as it were intuitively; that they can mimic their fellows; that some can depict the passions, and others describe them in words. You must have found some, who, apparently without particular attention or study, have the power of mimicry; so that when a person goes out of the room, the mimic can throw himself into his very attitude, assume his manner of speaking, and almost his manner of thinking. Just in the same way you find another man with genius (we call it "genius," as if it were something intuitive), who does not appear to be a more accurate observer than others, who yet, with the pencil in his hand, represents on canvass the nature, the form, the attitude, and the expression of another, so faithfully as to arouse the sympathies within you, and compel you to say that that man has the same talent as the first, but a different mode of showing it. So, again, while others cannot mimic, cannot use the pencil, know nothing of drawing, yet they can in words, with a pen, represent things as naturally as they are exhibited on the stage, and thus call forth the sympathy of all mankind.

Now this is not intuitive; this ought not to be called the effect of genius; it is the result of a close, minute, reiterated observation, and the only difference is, that it is not labour to the man who can accomplish it. He likes the pursuit, he indulges in it, he repeats it. That is the difference between such a man and a man who does not readily effect such results. Now I conceive that something artificial may be done towards communicating to you the right principle, as it were, for unveiling the powers of your mind, towards making you observant of all that passes in your own circle, the very expression, the very attitude of others, and leading you to that sort of acumen at last, which makes you better physicians, better observers of the influence of disease on the body. This is my excuse for taking up this subject, which seems to lead us from the common course of observation, but in the present course I trust I shall be able to show the influence of the bodily frame upon the mind, as well as the influence of the mind upon the bodily frame.

This, on a superficial view, would appear to be something like supporting materialism, and therefore I conceive my safest course is to draw your attention, in the first instance, to the influence of the common organs of sense. You will find, by attending to this subject, that an idea is produced in the mind by a certain condition, or a certain change in the organs of sense,—that is to say, when an object is visible, when the rays of light have been reflected from an object to the eye, there is nothing conveyed into the nerve itself, but an impression only is made upon it. Now the common idea is, that something is carried along the nerve, as if something had been actually received there, and yet those who have studied this subject most carefully, who have been great discoverers in science, and on that account must be depended on for their accuracy, have said that the nerves are unhandy engines for carrying ideas, that there is, in fact, nothing received when the outward organ is impressed, and therefore that the occurrence of an idea in the mind results from a relation established in nature, by the Author of our being, betwixt the intellectual principle within us, the brain, the nerve, the outward organ of sense, and the qualities of things externally. We can come to no other conclusion than this, that there is a relation established between certain things, but what the nature of that relation is we cannot say. Yet of this we are certain, that nothing is *received*. Well, then, if nothing be received, there must be a change in the organ, in the nerve, in the brain itself, and an influence at least produced upon the intellectual principle itself. If, then, every impression upon the hand, every impression upon the eye, every impression upon

the ear, though seeming to us at first to be something received from without, be proved only to be a change in the organ itself, we shall be less surprised at finding that a certain condition or emotion of the mind results from the state of the body, and that the corporeal frame is, as it were, a great organ influencing the mind, and intimately influenced by it.

The first step, then, of this demonstration, if I may give it that important character, is to show how the vital organs are sustained together, and to show the influence of these vital internal organs, first upon the exterior muscular apparatus, and then the mind.

Accordingly I have before me a demonstration sufficiently minute for that purpose, and you cannot think how anxiously a person in my situation desires to have, as it were, a point, a rest for the mind, in the shape of a demonstration. But I much fear, after all, notwithstanding the nicety of the present dissection, that I shall make little of it; that is to say, I am not convinced that you will see it well. Therefore, however nicely the dissection has been prepared, I must ask you in place of it to look upon the rude sketches before you. You will understand that we have here a representation of the viscera of the thorax, the heart, the lungs, the stomach, the trachea, the larynx, and the pharynx. Now what I particularly wish to draw your attention to, is the eighth pair. We ought, indeed, in the first place, in studying this nerve, to consider its just relation to the viscera. Here comparative anatomy comes to our aid, to prevent us from forming incorrect notions. I say it is an incorrect notion to suppose that the nerve leads off from the brain, and conveys all the energy of that organ to the parts led to, because I know full well, by tracing from the lower animals to the more complex, the powerful operation of the circulation and of respiration, where there is no such nerve, no such source of nervous energy in the head, in fact, at all.

The reflection naturally occurring from this (and we shall have occasion to recur to it more than once) is, that in the lower animals the nervous energy lies in the part or organ itself excited, and there is no reason to suppose that the fact is much otherwise in the higher animals. We naturally say, therefore, "Why this chord — the spinal marrow?" This leads to the supposition that there is an established relation between certain parts, between which this is the agent of communication. In fact, the great engines of circulation and respiration, the great organ for the first step of assimilation, are all connected together; not grouped together, not united by juxta-position, but connected through a nervous energy. I believe I ought to verify this statement, though I know with regard to most of you, that it is unnecessary for me to do so. The best way to establish the point is, to consider the case of a person apparently drowned. The person is taken out of the water without breath, without action of the parts concerned in breathing, without pulse: the mere dilatation of the lung, its repeated action, will bring on the action of the heart. Were it not so, there would be no possibility of resuscitation, because this first action of the heart does not cause a decarbonization of the blood, does not bring arterial blood into the heart through the body. All that it does at first is to restore a sympathy between the action of the heart and the lungs, when, by repeating, for a certain time the inflation and compression of the lung, the pulse returns. We find other instances of the heart coming into action after the lungs. You know the common experiment with a frog. If it be rendered apparently insensible and motionless, on being immersed in a respiratory gas the heart, being pricked with the point of a needle, begins to act, and, according to its nature, acts two or three times, when, being again pricked, it renews its action, and presently, after it has so acted, the breath returns, and respiration takes place, and when the animal has respired once or twice, it turns over from its back to its belly and leaps. Here you see the sympathetic action of the lungs drawing after it the action of the heart, and then again the action of the heart drawing after it the action of the lungs.

The experiments of Mr. Coleman in his early life tended to show that if a narcotic medicine were in the stomach during the attempt to perform such experiments, the attempt did not succeed, implying that those three organs, the lungs, the heart, and the stomach, are closely connected, not merely by proximity of situation, but by being supplied by the same pair, the par vagum, which lies by the side of the pharynx, giving off the pharyngeal branches. Coming to the larynx, it gives off the laryngeal branches. Coming down to the thorax, it sends down the recurrent under the arch of the thorax, which course removes the particular interest attaching to it, because so long as it continues to run from its source, distributing its branches, there is no intricacy about it, nor is there any call for its minute study. But you next find the nerve taking a circuitous course, the branches running up together, and it now becomes a fair subject for close examination, a fair subject for inference. Here we

have occasion to speak of a point which I have already once made the subject of remark, viz., the action of the trapezius muscle. We had reason to conclude that this muscle acted in the process of respiration, and in restoring exhausted respiration; but no part of it acts without being stimulated, or without some relation being established between it and other parts. Well, then, here is the nerve running down to the root of the lungs, about to expand on the lungs, but not before it sends circuitous branches back to supply those parts which are necessarily drawn in in the regular act of breathing; the frame, were they not so drawn in, (one branch being kept irregularly or imperfectly excited,) suffering spasm, and suffocation being the effect. Now the nerve comes down into the chest, and passes to the lungs, where it throws out little branches, which unite again, and so are distributed to the heart. Some persons formerly said that the lungs were not sensible bodies. No, they are not very sensible. So again has it been said that the heart is not sensible, nor is it so in the common acceptation of the term. We know full well that when HARVEY put his hand to the heart of a young nobleman which an injury had laid bare, the youth did not know that the organ was touched; he did not know that they were handling him at all unless they touched the external integuments. Thus it appears, in a common parlance view of the matter, that the heart is insensible. But is that the fact? We know that we cannot rise from our seat without a certain increase of the pulsating artery, nor can you accelerate your pace homewards without a corresponding acceleration of the action of the heart itself. In short, knowing that the heart is connected with every important vital organ, and affected by every condition of the body, you must be prepared to understand that the heart is acutely sensible to the influence of the mind, when labouring under passion and when in motion.

The parts within the thorax being thus connected with the stomach, they have a relation to the parts external, to the moving parts, and yet there are no nerves going out from the centre straight to the circumference, — no nerves strictly in union between the lungs, the heart, the stomach, and the exterior muscles. We must therefore go back to where the par vagum reached the throat, and see what other nerves there may be coming down from the neck, deviating only so far as they go to the muscles externally, instead of the parts internally. Here is a sketch which will bring to your recollection the number of powerful muscles that act in moving the chest. We have here, for example, the sterno-cleido-mastoideus, which is a muscle of the head; but if the head be fixed, it is a powerful muscle for raising the sternum and dilating the chest. We come down here again to the pectoralis major, a powerful muscle of the arm. Change the origin to the insertion; make the insertion the fixed point, and then the origin becomes the insertion,— that is to say, instead of moving the arm it dilates the thorax. Then we come round to this powerful muscle, the serratus major anticus, spreading like a hand upon all the ribs, taking its digitations from the moveable ribs, then forming a fleshy belly which runs under the shoulder and scapula, to be inserted into the basis of the scapula, and is a muscle of the scapula and shoulder; but if the shoulder be fixed, it is a muscle of the thorax. Thus you will observe that if the two parts be fixed, that is, the head above and the shoulder at the lower part, all those muscles which are muscles of volition and ordinary motion of the extremities, become muscles of respiration.

I beg your attention next to the trapezius. If we require the head to be thrown back and fixed, this is a powerful muscle for that purpose. If we require the scapula to be thrown back and fixed, that is the direct office of this muscle. So, then, this muscle, though it is not a muscle of the head, yet by fixing the head and shoulder it makes those parts the points from which the other muscles go off as from an origin, and this becomes a powerful muscle of inspiration. Indeed it is very much worth your observation, that nature, both by the distribution of the nerves and the application of powerful muscles, seems more anxious, if I may so speak, to give freedom and fulness to inspiration than to expiration. Your own feelings and observation point out this, that in all anxious states of breathing, in all excited conditions of the lungs in inspiration, the act of difficulty is the dilatation of the lungs. The elasticity of the parts, the compressibility of the whole, seems sufficient to expel the air.

Now, reviewing these muscles, let us attend to their action. The jockey is well aware that if he wish to ascertain the healthy condition of the lungs of the horse, he must give the animal a smart blow, when an immediate and powerful inspiration is made, so that inspiration in that case is the immediate effect of alarm. It is the same with man; if we are startled by anything, an instantaneous extension and dilatation of the chest takes place. Now this surely is not an accidental thing; for you will see at once that if there be a condition

of alarm, and a state of preparation of the body for avoiding the object of alarm, or for struggling against it, or defending ourselves, this is necessary to the force of the extremities. You may remind me, perhaps, that if a man cut his throat, the moment he has opened the trachea below the muscles of the glottis, the thorax subsides, and he feels as if struck with death, so feeble are his motions. Experiments of this kind have been made upon animals. If you open the trachea of a dog and throw him into a ditch, he will try to swim across, but he cannot do so, and sinks. But there is a preparation for action; the chest is dilated, and, being dilated, the muscles of the glottis keep it so, and give freedom to all the muscles of the chest, so that they are not, for the time, muscles of respiration, but muscles of the arm. The thorax is full, the muscles are placed in a position for the greatest possible action at the further extremity, and the whole animal is therefore in a state prepared for exertion.

What is the condition of a person in a state of asphyxia? See a man labouring under asthma, and at once you are led to understand his position. He does not stand upright, but leans upon the table, or rests his elbows there. He seeks in some way to make the shoulder the fixed point, because his alarm has obstructed respiration, and he calls in the whole muscular system to assist in effectively aiding the dilatation of the chest.

The next point that I would advert to—and this is purely professional—is the agency of the arteries. You know that the lungs and the arteries are occupied in one office, to produce and convey vital arterial blood into the system, to supply all the different functions, powers, and endowments of life. These are but two parts of one object,—the one the preparation of the blood, and the other its propulsion. When, therefore, a person is losing blood, he labours often under a symptom as if he would suffocate; he labours in his breathing; he heaves the chest; he has all the characters of anxious breathing; you see it in the workings of the chest, the elevation of the shoulder, the action of the heart, and the inflation of the nostrils. He exhibits extraordinary paleness, not merely becoming colourless because he is losing red blood fast, but because he is obviously labouring under the influence of difficult and alarmed respiration. There is a natural excitement of the respiratory organs attendant on the draining away of blood. Call to mind the figure of the dying gladiator. There we have the action of a man who is wounded, and from whom there is a draining of blood. The instrument which has wounded him is at his side. I beg your attention to the particular manner in which he lies. This statue is indeed true to nature, the best proof of which is, that it has been spared in all ages, as well by those savage nations that made inroads upon Rome, as by the refined nations and citizens of our own age. There must be something very true to nature where every one acknowledges the beauty of expression. The gladiator lies half recumbent, resting upon one hand, with the other upon his knee. Notice the condition of the shoulder; he feels that in the draining of blood there is a call upon the act of respiration. If it have ever been your lot to see a man dying in this condition, you must have observed that he clings to his assistants, hangs upon those who are really holding him up, and the shoulder is raised; all the muscles are in violent action, the chest is greatly dilated, and in that condition he labours until death actually takes place, and he falls, relaxed from his hold.

Look, again, at the group known by the name of the Laocoon. The artist, a man of fine and sensible imagination, has contrived to make the Laocoon restrain the expression of his pain, and with what admirable propriety! The whole group, indeed, has met with admiration. The true explanation of the restraint is to be found in the relation which the muscles have to the arm and the chest; they continually act in such manner that you cannot dilate the chest and move the arm at the same time. You must have observed that in wrestling or in any other condition of great exertion, it is impossible for a man to breathe during the action, nor is the voice audible. It is in the intervals of powerful action only that the voice is to be heard. The Laocoon has been entwined by the serpent, and is endeavouring by the most powerful exertion to tear the serpent from him. During the period that the arm is pushing off the offensive animal, it is impossible that he can speak, on account of the necessity to which I have before alluded; namely, because the powerful muscles which dilate the chest are now engaged in moving the arm. Hence the condition given to the figure. The artist, you perceive, is rigidly true to nature, and most powerful in the expression of character. The most surprising thing in the consideration of this subject is the accuracy which we all display in judging of what is natural in portraiture,—in judging of what we see, and acknowledging it to be rightly expressed, without knowing how. It reminds one of an actor in a tragedy. Some of you perhaps may recollect Mrs. Siddons in some of her finer scenes, where

the poet leaves her to her own conceptions and depiction of a character while asleep, in which seeming state she continued to move the hearts of every one beholding her. In the deepest characteristics of sleep there is occasionally, and was expressed by Mrs. Siddons, the convulsive throb through the whole frame, the action of the platysma myoides of the neck, a swelling of the nostrils, a movement of the lips, and a little obvious respiratory action. It was not so remarkable that she represented these effects, but that every one of the thousands who beheld her should feel the chord of sympathy vibrating in his own heart. It is remarkable that mankind should be so held together by sympathy. In the Laocoon, again, how evidently silent is the victim! The artist is in this, too, still true to nature, for who could cry out in receiving a mortal wound? The whole of the parts are in agony; and under the influence of approaching death it would be highly unnatural for an actor, who had reasoned on nature, to cry out at such a time.

So far with regard to the mere muscles, —the connexion simply between the muscles of the thorax and the muscles of the arm; but those muscles, which are an outward apparatus, though an organ of respiration and expression, have no direct connexion with the ribs, with the parts below. If, however, you trace back the par vagum to its source, you will find that there are other nerves coming down from the medulla oblongata.

Here is the phrenic nerve, acknowledged to be a nerve of respiration, which excites the diaphragm, the most important muscle in all the vital economy, into contraction, just at the moment of the dilatation of the chest in respiration. There is another nerve very near it, the external thoracic, which passes across the other nerves, but has nothing to do with them, and there are the intercostal nerves, running to the muscles. One would naturally say, why should there be two sources of nerves? Here are two, one running from the intercostal spaces to these muscles, and another running down from the neck, for the purpose, I should allege, of bringing this powerful muscle the serratus magnus into combination with the diaphragm. I observed that this was a powerful muscle of inspiration when the head was fixed, the sterno-cleido-mastoideus muscle drawing up the sternum and dilating the chest. These muscles, connected with the neck, have no power unless they are inverted, and their insertion is made their fixed point. Now if there be a stimulus acting on the trapezius, it draws back the head, draws back the shoulder, and gives a fixed point for the insertion of these other muscles, and therefore throws them all at once powerfully on the ribs as muscles of respiration. These nerves run down their course, the other nerves also uniting at their roots, both for the purpose of uniting these muscles into one combined action, and connecting those muscles with the internal viscera and their sensations.

But, perhaps, you have been thinking, how can this affect the process of respiration, such various actions being assumed in various conditions of the lungs? The condition of the heart and lungs is one thing, and produced by certain distinct nerves, but how is all this external part to be put into action? There must somewhere be a relation established between the external part and the internal, by which means the impressions felt within hold correspondence with the action without. Take an example from the passions, whether of grief, laughter, or fear. There is a species of expression of which you can easily comprehend the phenomena, that, for instance, of terror. When a man is terrified, you comprehend a great deal from his expression. You see that his eyes are fixed on the object of his alarm, you understand why his eyes roll and are largely uncovered, you know by his motions that he is in a condition of fright, and anxious to run from the object of it. All this is easy to the comprehension; it exhibits the direct influence of the mind on the body; the man is intent upon the object before him, he is prepared to escape or to defend himself, which is natural enough, but you probably do not know why his shoulder is so elevated, or his chest is so dilated that he cannot breathe,—why there is that awful condition of all the respiratory organs which prevents him from propelling his breath freely,—why he is cut in his speech and cannot utter a word, —why there is that extraordinary paleness, that blanched cheek and livid lip, —and far less why there is that convulsion of the neck and throat, that inflation of the lips and nostrils, so peculiar on an occasion of terror. But if you observe the knocking at his side, if you notice that the condition of his mind is obviously influencing the action of his heart, obviously influencing all those viscera that are grouped together, you discover that they must have an influence on these muscles, and that an excitement is first produced internally, and then reflected on the muscles of respiration, by which he is thrown into all the apparent agony during which the respiration is obstructed, and the speech is checked. You have another proof of the source from which this expression arises in the circulation, evidenced by paleness and

coldness of the surface. By thus following up the appearances presented by the passions, and studying their source and relations, you acquire not only a new interest in an old subject, but you really have new means afforded you of increasing your acumen in discriminating the symptoms of disease. I will go on with this subject a little farther at the next lecture.

CLINICAL OBSERVATIONS

ON

VARIOUS DISEASES,

BY

MR. WARDROP.

CASES OF RETENTION OF URINE.

Whenever there is a retention of urine, the first endeavour of the surgeon usually is to introduce a catheter, to procure the evacuation of the retained fluid, thus disregarding the immediate cause which has prevented the bladder from performing its natural function. Now there are, in particular, two states of disease, either of which predisposes to retention of urine; these are, an enlarged prostate, and a strictured urethra, and in both these affections, if inflammation supervene, the retention follows. It is such cases of retention as arise from the one or the other of these two causes, that the following instances are designed to illustrate.

Case 1.—I was sent for early one morning to draw off the water of an old general officer who was suffering from retention of urine. Sir Gilbert Blane had previously visited him, and had proposed the use of the catheter. The urinary bladder was considerably distended, no water having passed since the previous evening. The prostate gland was enlarged, and the temperature of the internal surface of the rectum was much increased. The patient's pulse was not sensibly affected, and his skin was hot. He was not aware of ever having had any disease in the affected parts, although he had long felt a difficulty in voiding his urine after free drinking, and he attributed the present attack to having drunk freely the night previously.

In place of endeavouring to introduce an instrument into the bladder in order to withdraw the urine, I proposed first to subdue the inflamed state of the prostate and parts around the neck of the bladder. For this purpose blood was immediately taken from the arm, while he was in the horizontal position, and the current was allowed to flow until the pulse was quite subdued. He was by this measure relieved very quickly from all pain, and in a few hours the urine began to ooze from the urethra. Twenty leeches were now applied to the perineum, and in a few hours more the urine, after having come *guttatim*, escaped in a stream, and the bladder was completely evacuated. Nothing but the antiphlogistic regimen was afterwards employed, and in a few days the functions of the urinary organs were completely restored. This patient lived several years after, and I had occasional opportunities of seeing him, and I learnt that he never had a subsequent attack of the same kind.

Case 2.—I visited a gentleman who, in the eighty-second year of his age, was attacked with retention of urine, for the relief of which repeated attempts had been unsuccessfully made to introduce a catheter. I found the bladder considerably distended. There was great tenderness and pain in the pelvic region, the action of the heart and arteries was much accelerated, the skin was hot, and he suffered great disquietude. I endeavoured, without using any force, to introduce a catheter, but the instrument stopped at the neck of the bladder. I found the prostate gland enlarged and tender, with increased heat in the adjacent parts, and I was informed that he had long had an enlarged prostate, in consequence of which instruments had, on several occasions, been used. Persuaded that the catheter could not be introduced with force, and perhaps not without more or less injury, and that the retention of urine had been caused by an attack of inflammation, I conceived it expedient to subdue that attack before again endeavouring to draw off the water, and I was also not without hope, that after the inflammation was subdued, the urine might pass along the canal of the urethra, an event which had occurred in the former case. Forty ounces of blood were taken from the veins of both arms before syncope supervened. After the faintness had passed off he rose from the bed, and passed a few spoonfuls of urine, and during the subsequent eight hours he remained tranquil and undisturbed, though no more urine escaped. I therefore now endeavoured to introduce a catheter, when the instrument passed without difficulty or pain, and continued to be passed with perfect ease, as long as the bladder was unable to perform its functions.

Case 3.—I visited Colonel ———, who had not voided his urine for ten hours. His countenance expressed great anxiety,

his pulse was much excited, and he had a good deal of fever. He stated that eighteen years ago he was under the care of Sir Everard Home for strictures of the urethra, then having three strictures, each of which was destroyed by caustic. He added that he returned to India in perfect health, introducing a bougie occasionally for some years, but that no bougie had passed for the last ten years, and that during the last few months he had had several attacks of retention, the stream of urine having been for a considerable time preternaturally small. He had used no remedies except during the periods of retention, on which occasions no instruments could be passed into the bladder, the attacks yielding to large doses of opium, and repeated local and general bleedings. He enjoyed perfect general health, and had been in India forty-five years.

After placing him in bed, I opened a vein in his arm, and abstracted upwards of forty ounces of blood before the pulse sank. He had already taken sixty drops of laudanum, and sixty more were now given. Two hours after, the urine began to escape. He then took six grains of calomel, and passed a better night than usual. On the following day the size of the stream was larger than it had been for some time, and the irritation of the urethra was so much diminished that he was called on to make water only once during the next night. Notwithstanding the depletion, and the rigid antiphlogistic regimen which was pursued for some days, he experienced no diminution of his muscular strength. The effects of this attack being in a few days subdued, I succeeded, after numerous careful attempts, in introducing the smallest-sized elastic-gum catheter into the bladder, where it was retained, and a succession of instruments of larger and larger sizes were afterwards introduced, so that in a few weeks the contracted urethra was dilated, and now, after the lapse of several years, I have learned that he continues well.

Case 4.—A married lady, upwards of thirty years of age, after having suffered much pain and irritation in the parts around the external orifice of the vagina, was attacked with retention of urine. I found the bladder very much distended, accompanied by a great deal of febrile excitement. A catheter passed readily along the urethra till it reached near the bladder, where it met with a firm resistance. Instead of using any force, I withdrew the catheter, conceiving that the inflamed state of the parts and the spasmodic contraction of the urethra had caused the obstruction. I now opened a vein in the arm, and allowed the blood to flow until syncope supervened, which did not take place until thirty ounces of blood were abstracted. In two hours I visited her again, when I had the satisfaction to find, that the urine had been escaping in small quantities. Its flow gradually increased, and the bladder was completely emptied. The bowels were copiously evacuated, and in a few days her health was restored.

Remarks.—These cases will, I hope, suffice to point out the practical importance of duly deliberating on such occasions, before adopting or persisting in the use of the catheter or knife as the means of relief. They teach us the propriety of endeavouring to discover the cause of the retention of urine, before employing any mechanical means for evacuating the bladder. Whatever may be the precise nature or extent of disease in the urinary passages, when an accidental retention takes place, the inability to void the urine must arise only from some temporary cause, and therefore it ought to be the first duty of the surgeon to trace what that cause is, and endeavour to remove it. In most instances the retention will be found to be the effect of a temporary inflammation, which inflammation is *usually* brought on by the *abuse of spirituous liquors*,—by some over-excitement of the organs,—or by some disturbed state of the system generally.

CASE OF

TUMOUR OF THE ABDOMEN

CONNECTED WITH REMARKABLE

DISEASE OF THE LUNGS.

By William Tomkins, *Esq.*, *M.R.C.S.* &c., *Yeovil, Somerset.*

Case.—I was applied to by Charles Parker, ætat. 28, of delicate make and strumous habit, on account of a protrusion from the abdomen, which he conceived was a ventral hernia. He informed me that he discerned the tumour for the first time about two months previously, after violent walking exercise. It was then small, and slightly painful, but it had gradually increased, and now appeared about the size of a hen's egg, without the slightest discoloration or tenderness of the skin. On examining the swelling, it presented to the eye the precise appearance of a ventral hernia. It protruded midway between the umbilicus and the spine of the ileum on the right side. I laid the patient in a

recumbent posture and applied pressure, on which the swelling instantly receded with a slight crackling noise; but the man complained of faintness, palpitations, a sense of suffocation, and tickling in the throat, which I could not exactly account for, as he did not appear to have any timidity about him. I desired him to stand up and cough, and on his doing so, the swelling reappeared immediately.

As I was not satisfied respecting the nature of the complaint, I desired the patient to call on me again in about ten days, which he did. I then found that the tumour was nearly double its original size, but still capable of being returned easily on applying pressure in the recumbent position. The general health of the patient was tolerably good; indeed he complained of nothing but slight dyspnœa on walking against hill. I now felt satisfied that the swelling contained a fluid, and, therefore, determined to make an opening with a couching needle, in the manner recommended by the late Mr. Hey in cases where there was doubt. I did so, and on withdrawing the needle a "*thin serous fluid*" escaped. I then dilated the wound with a bistoury, and let out about twelve ounces. I applied a dressing of adhesive plaster, and pressure, and the wound healed by adhesion in a few days.

About a month elapsed before the man came to me again, when I found the swelling larger and more diffused. I opened it and let out a pint of fluid similar to that before evacuated. On examining the cyst with a probe I could not trace any communication between it and the abdominal cavity, or any sinus beyond its apparent parietes. I therefore determined to pass two setons, one from below upwards and another across, and in a few days there was a good purulent discharge, which gradually lessened, and after a month I was enabled to remove the setons, and had the satisfaction to see the wound perfectly healed and the cyst adherent.

I lost sight of the patient for nearly six months; he had returned to his work, and felt but little inconvenience from his complaint; now, however, he complained of cough and dyspnœa, which he said had come on gradually for about ten days, and a return of the tumour, accompanied by much more pain than he had before felt. He had quick pulse, slight expectorations, and night perspirations. There was little sound in the right side of the thorax on percussion. Coughing produced a swelling in the site of the original tumour, which, when returned by means of pressure in the recumbent posture, excited cough, dyspnœa, palpitation, quick pulse, and faintness. I applied such medical treatment from day to day as I thought the circumstances of the patient required, and which it would be tedious to detail. In a short time the abdominal swelling gave way at the lower cicatrix left by one of the setons; the cough directly became more troublesome, and whenever it occurred, a volume of air rushed through the wound, which I found it impossible to prevent by any means that I could devise. I tried oiled silk covered by adhesive plasters, bandages, &c.

My attention was now called to a new feature of the case. The man complained of a most acute pain, together with a swelling by the side of the breast bone, which passed in and out, as he breathed, with a crackling noise. On examination, I found a protrusion of the lung under the skin, which could be distinctly felt to crepitate between the fingers. It was about the size of a hen's egg, and passed out just under the second rib on the right side, close to its sternal articulation. I tried to restrain this pulmonary hernia, if I may so term it, by means of pressure, but the man could not long endure it, and would not keep anything over the part. The general health now declined, and it was evident that the right lung was much diseased. The cough and expectoration were considerable, as well as the discharge and escape of air from the abdominal ulcer, which had now extended itself to the extent of several inches, and assumed a very unhealthy appearance.

I need not particularize the progress of the case from time to time; suffice it to say, that the pulmonary symptoms gradually increased for about three months, when the rupture of the lung, which had considerably enlarged and had adhered to and inflamed the skin for a fortnight, gave way, and an external opening was found, through which a purulent discharge passed. This opening gradually extended itself from day to day, and on the fifth day from its giving way, the patient respired and expired almost as freely through the wounds in the abdomen and thorax, as he did by the trachea. It was impossible to prevent this, though I tried every means, as the rush of air and the discharge from the lungs removed everything I could apply externally. Several of my professional friends saw the case with me, and it is astonishing that the patient lived upwards of three weeks in this situation. Mr. Latham, my assistant, and myself, opened the body in the presence of Dr. Penkivil, an intelligent and respectable physician of this town, and the following were the

Post-mortem appearances.

The right lung was completely consolidated, and united to the thorax by inseparable adhesions. The sternal end of the second rib was absorbed, where the portion of lung forming the hernia had protruded. There was a large cyst in the centre of the right lung, empty, but of sufficient size to contain four ounces of pus. From the lower part of this cyst ran a sinus, which passed down behind the diaphragm as low as the eighth rib, an inch of which was completely absorbed, and a separated portion of half an inch was lying loose; at this place the matter had made its way out, and there ran down under the skin to the spot where the tumour was first observed. The heart was of its usual size, but the pericardium was thickened, and contained two ounces or more of pure pus. The abdominal viscera were healthy, as was the left lung.

Remarks.—The perusal of this case must present to the reader many striking and remarkable features. It is in the first place very extraordinary that the abscess of the lung should have proceeded so far, and even have pointed in the abdomen, when, excepting the dyspnœa, there was not the slightest indication of pulmonary disease; but cases of extensive and even fatal disease of the lungs do sometimes occur in very strumous habits, and proceed almost to their termination before the urgency of the symptoms induces the patient to apply for professional aid; indeed such instances have frequently come under my notice. The great similarity of the swelling in the first instance to a ventral hernia, and the crepitating feel, produced most probably by an escape from the air-cells, are well worthy of remark, and show us the importance of an attentive and well-considered examination in every case of tumour, before venturing on an operation. Again, the healing of the cyst and the non-accumulation of any fluid in the part for several months, is a fact to me quite inexplicable, as it was evident from the post-mortem examination that neither the cyst in the lung or the sinus leading from it could have closed; and yet as the patient was for months free from cough or expectoration, we cannot imagine (coupling these circumstances with the lower cyst remaining impervious) that any accumulation could have taken place in the lung, or how comes it that it did not gravitate? Are we then to infer that such cysts do secrete? I presume so, and if we admit the fact, what suspended its action? It is a most curious circumstance that a portion of the lung should have protruded after the cyst had a free vent by a dependent opening, and it is more singular that the portion of lung which protruded felt perfectly soft and healthy for at least seven or eight weeks after it was first discovered. There would have been nothing remarkable if purulent matter had pressed on the rib from an abscess of the lung and caused its absorption; but for a healthy portion of the lung to effect this, is to me a circumstance of a very unaccountable nature. The length of time the patient lived after the two external openings of communication were formed with the lungs, is another proof of the extraordinary powers which nature frequently exhibits to us in her modes of accommodation to the influence of diseased actions and altered structures. The quantity of purulent matter in the pericardium was a circumstance of very rare occurrence, and it is particularly worthy of notice that during life there were no marked symptoms of such disorder. I do not recollect ever to have felt the slightest intermission of the pulse, except when the palpitations and faintness occurred from my making pressure on the tumour in the abdomen.

Having said thus much, I leave this (I presume unique) case to the reflections and observations of the numerous, intelligent, and respectable readers of that excellent and straightforward publication, THE LANCET.

Yeovil, Somerset, August 12th, 1834.

ON THE TREATMENT OF

MALIGNANT CHOLERA,

CHIEFLY WITH

EMETICS OF COMMON SALT.

By GEORGE BEAMAN, Esq., M. R. C. S., *London.*

DURING the last three months I have attended several cases of cholera, and the following plan of treatment has been so extremely successful that I am induced to trouble the Editor of THE LANCET with this communication, hoping it may benefit some of my professional brethren.

The symptoms of cholera in its worst form are too well known to require description, and to this stage of the disease, I wish it to be understood, the following plan of treatment is especially applicable.

To an adult I give three tablespoonfuls of common table salt in half a pint of tepid or cold water; this produces vomiting in less than one minute, and the vomiting ought to be powerfully violent; such effect is followed by an immediate

diminution of the cramps, and very marked increase in the fulness of the pulse; the other symptoms are much amended, the voice becoming stronger, and the muscular power greater. It may happen that the first dose answers every purpose; frequently, however, the beneficial effect of the first dose is not permanent, for within 20 minutes, or half an hour, the pulse begins to flag, and then rapidly decreases in power, another attack of cramp ensues, and the prostration increases; a second dose of the salt is now to be administered, and even a third may be required. I have never had occasion to give it oftener than three times.

The good effect of the salt emetic really depends upon its producing powerful and very efficient vomiting; without this full operation it ought to be thrice repeated, with an interval not exceeding five minutes between the doses.

The adjuvant treatment consists in placing the patient between warm blankets, but he must not be too heavily covered—rubbing the parts spasmodically affected with stimulating liniment, and occasionally applying a mustard cataplasm to the pit of the stomach.

As medicine, I dissolve half a drachm of carbonate of soda in two or three ounces of water, and with the addition of four or five drams of fresh lemon juice give it while effervescing, every hour, for three or four doses; afterwards once in four hours. For 48 hours I give no other medicine.

I have tried, in two instances, scruple doses of the muriate of soda with the same quantity of the carbonate; but the combination was nauseous to the patients, and no advantage was gained.

Generally on the day after the attack the patient passes a small fecal evacuation, containing bile, otherwise I give, at the expiration of 48 hours, a drachm of castor oil, or a few grains of rhubarb.

The active part of the above treatment requires the constant superintendence and watching of the medical practitioner from two to four hours, during which time I give the patient as much water, or toast and water, cold or tepid, as may be wished for, and no other beverage. For the next 24 hours I allow only weak black tea and thin arrow-root; and on the ensuing day a little broth or light pudding, my patient now being generally convalescent; indeed it is remarkable how rapidly recovery ensues, considering the late direful state of the individual.

The plan may require some modification when treating young or aged persons.

In conclusion, I beg leave to remark, that I claim no merit for originality of system; but as I communicate important practical facts relative to the successful treatment of no less than eleven consecutive cases of this disease in its worst form, I hope my professional brethren will consider that I am actuated by no other motive than a wish to benefit our fellow creatures.

King-street, Covent-garden,
Aug. 19, 1834.

EXPERIMENTS

ON

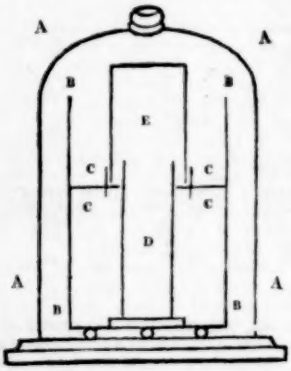
THE BLOOD.

By William Reid Clanny, *M.D.*,
Sunderland.

In attempting to extricate carbonic acid gas from animal fluids, I was at first compelled, for reasons given in this paper, to employ Vogel's apparatus, which I have brought to as much perfection as it was capable of attaining; but I discovered that the process for extricating this gas from animal fluids by this instrument was very tedious, and that the delay and agitation in working the air-pump, caused the free carbon and the free oxygen gas of the blood to unite, and form an additional quantity of carbonic acid gas, whether I operated upon arterial or venal blood. Besides, I could never assure myself that I was correct, when I attempted to deduce quantities of absorbable gases from the weight of precipitates formed.

I commence this paper with an account of my newly-invented apparatus for obtaining gases from blood, which has cost me much deep reflection and anxiety, and which has always been a desideratum for the chemical investigation of the gases which enter into the composition of the fluids of the animal, vegetable, or mineral kingdoms. My design was to accomplish the extrication of gases from fluids within the air-pump exhausted receiver in such a manner, that those gases might afterwards be submitted to analysis, being aware that the plan devised and carried into effect by Vogel was objectionable, from the inaccuracies it permitted as respects its only use, viz., for ascertaining the carbonic acid gas of the blood. Besides, several eminent chemists could never use this instrument so as to obtain any carbonic acid gas from that fluid. It must not, however, be denied, that this was the only one which could be used with any propriety in such experiments, as I have explained elsewhere in this paper. It was the use of Vogel's instrument which first turned my attention to the subject of gases in the blood.

The air-pump pneumatic apparatus is composed of the following pieces:—A tin cylindrical water bath, twelve inches in height, and nine inches in diameter, in which a moveable pneumatic shelf is placed upon a ledge; through the centre of this shelf there is an aperture for receiving the vessel (whether a flask or a jar) which contains the blood for investigation. Round this aperture in the shelf there are several others, to facilitate the extrication of any gases which may, possibly, be left in the water of the water-bath, and to expedite the extrication of the gases in the blood, by taking off the pressure of the atmospheric air by the agency of the air-pump. Here I beg leave to remark, that at the commencement of my experiments I always remove by the air-pump any gases which may be suspended in the distilled water which I use in this water-bath.



Let A A A A represent the exhausted receiver of the air-pump. B B B B the water-bath, supported by three balls, resting upon the air-pump plate, for the ready permission of the extrication of atmospherical air from the exhausted receiver. C C C C the pneumatic shelf, which rests upon a ledge within the water-bath. D the pneumatic flask or jar, graduated for fluid ounces. E the inverted pneumatic jar, graduated for cubic inches, and which is supported by the pneumatic shelf C C C C. When in use, the water-bath is filled half way up from the shelf to the letters B B, with distilled water, freed from gases. When the air-pump is worked, the atmospheric air is extracted from the exhausted receiver at the centre of the air-pump plate, which is kept as free as may be, by the removal of the button usually placed there.

For common purposes a graduated jar, seven inches in height and three inches in diameter, is used, but when blood is under examination, I employ a graduated, cupped, and stop-cocked flask, three and a half inches in diameter, over which a pneumatic jar, graduated in 100ths of a cubic inch, is inverted, resting upon the shelf. This jar is seven and a half inches in height, and four inches in diameter. The exhausted receiver of the air-pump is fifteen inches in height and ten inches in diameter.

Having filled the air-pump pneumatic trough to the mark above the shelf, place the trough with its contents in the exhausted receiver, and extract any gases which may be contained in the water, by the agency of the air-pump, and then remove the trough from the air-pump to be in readiness for use. Pour into a twenty-ounce capped and graduated flask, for receiving blood or other fluid for analysis, ten ounces of distilled water, and also fill the pneumatic jar of thirty ounces capacity (which is to be placed over this flask) with distilled water; place these vessels in the exhausted receiver of the air-pump, and extract any gases which may be contained in the water. This being done, arrange the flask containing the ten ounces of water, and remove the atmospheric air from it, and receive ten ounces of blood intended for experiments, in vacuo, as formerly explained in the pages of THE LANCET. The flask being now filled with about equal quantities of blood and of water, must be conveyed to the air-pump pneumatic trough, and having placed over the flask the above-named jar, containing water, put the trough with its contents into the exhausted receiver of the air-pump, and upon working that instrument, as soon and as energetically as possible, the gases will immediately ascend into the pneumatic jar in a rapid and beautiful manner. I find it needful from time to time to admit a little atmospherical air into the receiver, as the expanded gases from rarefication, frequently occupy more than the half of the pneumatic jar, and, consequently, the water of the water-bath does not retain its proper level. This does not prevent the continued extrication of the gases.

It will readily be understood that in this process the blood being experimented upon in an isolated state, in respect to the atmospherical air, the gases are extricated in a rapid and accurate manner. It will also appear, that by the blood having been received not only in vacuo but also in water, its particles are so divided, that the fibrine is not permitted to form those celules in its coagulation, which in

a great measure cause the retention of the gases of the blood, even under the action of the air-pump, as I have often observed when experimenting *à la* Vogel.

By my process the blood very readily yields all its gases, as I have proved by the performance of a diversity of experiments upon it after it had been submitted to the cold, though powerful distillatory process, by the agency of the air-pump.

All chemists are aware, that when we employ heat in analyses of animal fluids, we readily coagulate the albumen, should there be any present; besides, we give origin, by the agency of heat, to a set of bodies which had no existence in the subject of experiment, the ultimate elements of which are thus disunited, and are recombined in a new manner. We know, in respect to animal fluids, that wherever free carbon meets with oxygen gas, an immediate union takes place, and carbonic acid gas is formed, whether agitation be employed or not.

This new instrument was, in the first instance, experimented with in the presence of my friend, Mr. Edward Haygarth Maling, M.R.C.S., who, being a competent judge, at once expressed to me his entire approbation of it, and I do not hesitate to remark that we shall henceforth require no other for such investigations.

I will now present the results of many hours anxious inquiry. I give the names of persons whose blood was experimented upon, in the order in which they stand upon our Infirmary books, and I may remark that in none of them had I reason to consider that the blood was otherwise than what the French authors call "normal," from *normalis*. To Mr. Penman, our late house-surgeon, I am greatly indebted for his continued attention during my investigations. Ten ounces of blood were used for experiments in each case.

Venal Blood.

	J. Wilkinson, October 11, 1833.	J. West, June 6, 1834.	H. Foster, June 9, 1834.
Oxygen gas. Cubic inch.....	.02	.03	.02
Carbonic acid gas, ditto	.06	.06	.05
Nitrogen gas, ditto	.58	.64	.62
	.66	.73	.69

Arterial Blood.

	Mary Jackson, Jan. 1, 1834.	W. Carr, April 7, 1834.	H. Foster,* June 2, 1834.
Oxygen gas. Cubic inch.....	.06	.05	.06
Carbonic acid gas, ditto	.02	.02	.03
Nitrogen gas, ditto	1.89	1.80	1.84
	1.97	1.87	1.93

The uniformity, as to quantity and proportions of gases, is astonishing. We find that the difference of tint in venal and arterial blood is not to be attributed to the oxygen in the arterial blood, but to the excess of free carbon, which I have proved to be always present in venal blood.

We are aware that in animals with red blood, the principal effect of the mutual reaction between atmospherical air and blood falls upon the colouring matter. The serum, without the presence of colouring matter, converts equally well a portion of oxygen gas into carbonic acid gas, but this quantity is very insignificant compared with that which disappears when serum is mixed with colouring matter, the brown colour of which passes to vermilion red. It is believed that this is effected likewise by the oxydation of the iron, which I well know, by experiment, enters into the composition of the crassamentum of the blood, but this I have proved to be an error; besides, neither the brown tint nor the vermilion red has a shade which can be attributed to the presence of oxides of iron. I have performed several experiments upon the crassamentum of the blood, in order to ascertain the nature and quantity of iron which always enters into its composition, but my experiments are not yet so extensive nor so decisive as to be laid before the public.

Medical men have been too often led into error by experimenting upon tortured animals, and upon substances recently removed from animals killed with a view to such experiments, and also from human bodies after sudden death. In my opinion such experiments ought only to be considered as curious anomalies, and to-

* The same person from whom the venal blood was taken.

tally inadequate to explain the phenomena of animal life. Besides, I have such a horror of cruelty, that I never injured the smallest animal in any experiments, being always satisfied that by such proceedings I was not the person who could thereby advance the interests of humanity, or of medical science.

From the experiments just detailed, we are possessed of one fact that cannot be gainsayed—viz. that though there are only small quantities of oxygen gas and of carbonic acid gas circulating in our blood-vessels, there is always a large and uniform quantity of nitrogen in that fluid. From this we are compelled to infer, that these gases are absorbed by the capillaries of the pulmonary veins, for we know that the former gases are readily transmitted by living membranes. This is not the case with nitrogen gas, but it does not apply when capillaries are in action, because we know that capillaries take up fluids and gases indiscriminately—at least we know nothing to the contrary.

All my experiments upon this subject have shown me the greatest uniformity in the works of nature, and if membranes are the media by which gases are transmitted, we should not have the uniformity which we now know obtains, in respect to the gases and free carbon in the blood; besides, we know that from whatever cause the bloodvessels become enlarged in their caliber in any part of the body, we have generally an increase of heat in that part of the system, and in some instances to an astonishing extent, as in the case of Professor Thompson, detailed by himself in his usual admirable manner at page 26 et seq. of Vol. II. of his "Annals of Philosophy." The paper is entitled, "On the heat evolved during inflammation of the human body."

The rationale is this; the free carbon and oxygen of blood are in greater extent, though in transitu, in distended vessels, and more heat is thereby given out; besides, we know that increase of heat causes a greater facility of union between these two elements, and to which pain and irritation in cases of inflammation lend their assistance. From these experiments, we can understand the reason of the great uniformity of temperature which prevails over the whole system, and we are led to conjecture that part of the free carbon of the blood suspended in the halitus which proceeds from the surface of the air-cells of the lungs, comes *there* into contact with the oxygen of the inhaled atmospherical air, and is in the air-cells of the lungs converted into carbonic acid gas, whilst this *halitus* equalizes the temperature of the air-cells and parenchyma of the lungs in such a manner that it is little, if at all, higher than that of the body. If my experiments be found to be correct, and of which I have not a shadow of doubt, we are assured that free carbon at the moment it is given out in a direct manner through the membranes composing the air-cells of the lungs as an excrementitious substance, affords also, *in transitu*, animal heat to by far the most extensive surface of the whole body, viz. the air-cells of the lungs. Were atmospherical air absorbed by the air-cells only, we should have very little nitrogen in the bloodvessels, and a much larger portion of oxygen and carbonic acid gases. Besides, deleterious gases, which are more readily transmissible in this way, would find a ready access to the blood; but this is not the case, and is a subject matter for felicitation, for we know that many of the most readily transmissible gases through membranes, such as sulphuretted hydrogen, cyanogen, carbonic acid, nitrous oxide, &c., would cause immediate death did they find such a ready adit to the blood. Let us reflect that fishes in general absorb atmospherical air, which salt as well as fresh water contains; and though the quantity is very small compared with our immense atmosphere, still it is sufficient for some very large fishes, and as all red blood must possess *free carbon*, we are not aware of any other method by which fishes discharge this element, except by the gills, which must be in a direct manner into the water which surrounds the animal.

The theory of Priestley and Lavoisier, and, after them, of La Place, Crawford, Gren, and Girtanner, viz., that "the blood absorbs no air from the lungs, but gives out hydrogen and carbon, which, combining with the oxygen of the air inspired, form water and carbonic acid," is untenable, because, as I have proved by a series of experiments, air is absorbed into the circulation from the capillaries of the lungs, and free hydrogen is never given out by the lungs, and if it were we know of no process by which hydrogen and oxygen gases can be formed into water in any part of the human body at the temperature of blood, i. e. from 90° to 100°, for we know that oxygen and hydrogen gases cannot combine at ordinary temperatures, and may, therefore, be kept for any length of time in a state of mixture, without even gradual combination taking place between them.

Having in the pages of THE LANCET demonstrated that venal blood as it circulates, contains about 32 grains per thousand of free carbon, and that when it is left for 24 hours in vacuo, certain portions of carbon may, day after day, be removed

by the agency of the oxygen of the atmosphere till the 8th day, when no more can be removed by this agency, I have come to the conclusion that this removal is permitted by the gradual decomposition which the blood undergoes when in vacuo, as its texture appears under such circumstances to be broken down, and the smell becomes offensive in a gradual manner during the investigation. The claret or logwood tint is not in the least changed during the last two or three days.

In order to arrive at a correct idea with respect to the points under consideration, it is needful that we now enter upon the subject of the pabulum sanguinis chyle.

Reuss and Emmet were the first to give us a good analysis of chyle. These chemists remark, that this fluid, as it flows in the lacteals, resembles milk, being viscous to the touch, but does not coagulate when in contact with the air, like chyle drawn from the large lymphatic trunks.

The researches of Tiedemann and Gmelin are valuable, and correspond with those of the above-named chemists. Tiedemann and Gmelin found that after the chyle had passed the first series of glands, it rarely coagulated when left in contact with the air. After the chyle had passed those glands, its tint was changed from milk-white to yellow-red. The milky appearance of the chyle originates from the fat contained in the element, which being mixed with the bile and the pancreatic juice, forms an emulsion, of which the absorbents take possession. This is more remarkable after the animal has partaken of fat food, and is not to be met with if the food contain no fat substance, as when there is no flow of bile we find that the fat does not undergo the emulsion process, and in such case the chyle appears to be essentially changed.

If we agitate healthy chyle with a sufficient quantity of ether, the fat will be taken up by it, and the remaining fluid will become clear. The fluid contained in the thoracic duct of a horse which was killed during the process of digestion of oats, had a white-red tint, was milky in appearance, and coagulated in a few minutes. The cake was at first pale-red, but some minutes afterwards it became cinnabar-red. The serum was muddy, yellow-red, and, upon being left untouched, is deposited a colouring matter in the form of a fine powder. In four hours the serum was completely separated from the cake. The moist cake of the chyle weighed 3.01, and after desiccation 0.78. The residue, 96.99 serum, left after evaporation 7.39 per cent of the whole weight when it was dried.

Boiling alcohol removed from the cake a small quantity of yellow-brown oil. The serum was rendered clear when treated with ether. This liquid removed from it a mixture of elaine and stearine, which remained after evaporation.

The serum of the chyle, after it was experimented upon as formerly upon the blood, gave products analogous to those of that fluid.

One hundred parts of dry residue contained the following proportionals.*

Brown fat extracted by the first portions of alcohol	15.47
Yellow fat extracted lastly	6.35
Extract of food, lactate of soda, chloride of soda crystallized in octahedrons, probably by means of an animal substance	16.02
Extractive matter soluble in water (insoluble in alcohol) with carbonate of soda and a truce of phosphate of soda	2.76
Albumen	55.25
Carbonate of lime and a truce of phosphate of lime, obtained by combustion of the albumen	2.76
	98.61

From this fluid we can readily understand that the carbon of the blood originates from the chyle only.

How wonderfully the free oxygen, the free carbonic acid, and free nitrogen gases, are arranged in the venal and arterial blood!

In both of these we find a sufficiency of oxygen gas, from which, by its union with the free carbon, an abundance of carbonic acid gas is formed. We find also, that only about one-half of the quantity of oxygen gas circulates in the venal compared with what is met with in the arterial blood; and that at least double the quantity of carbonic acid gas, formed from the above-mentioned union, was always met with in venal blood. Hence the formation and equal distribution of animal heat throughout the whole system. In respect to the nitrogen, we find that arterial blood contains at least twice as much of this gas as we meet with in venal blood; and no doubt this gas not only facilitates the circulation of both descriptions of blood, but also contributes to the forces employed in the secretions, and also enters largely into the composition of fibrine or the fibre of all animals.

In a word, this gas must be considered as one of the most wonderful of elements, for by its agency the functions of life and of nutrition are mainly performed.

Sunderland, August 4, 1834.

* Vide Berzelius' "Organic Chemistry," French edition.

TREATMENT OF

ACUTE TONSILLITIS.

To the Editor of THE LANCET.

SIR,—Mr. Horne, in his communication to you on the use of the nitrate of silver in acute tonsillitis, seems to be of opinion that the nitrate enjoys a superiority over any other plan of treatment hitherto adopted in similar cases; but, Sir, with your permission, I will detail a case or two in order to bring under notice a still more efficacious remedy in that complaint; such, at least, I have lately found it in those instances which have afforded me an opportunity of using it.

Case 1.—On the 11th of April, 1834, I was requested to visit Jane Ward, ætat. 20 years, of rather slender make and scrofulous constitution, who had been under treatment since the latter end of March. Treatment: *V. S. ad* j *lb. Pulv. Jalap.* grs. xv; *Submur. Hydr.* grs. v. Misce. Fiat P. C. I promised to visit her in the evening for the purpose of lancing the tonsils, but was prevented. 12th. Cut both tonsils horizontally with a dissecting scalpel to a considerable depth. Ordered the patient to gargle the mouth and throat frequently with warm water so long as any bleeding continued. This patient was up and stirring about the house on the 13th.

Case 2.—On the 25th of April, 1834, I was called to Jane ...., upwards of 20 years of age, of a full and robust habit of body; both tonsils were exceedingly enlarged and inflamed, threatening suffocation and occasioning delirium. V.S. ad j lb.; both tonsils were cut as in the above case. Patient says that she has found great relief, the sense of suffocation being entirely removed, and deglutition being almost as easy as during health. This patient left her bed, and went into an out-kitchen on the 3rd day. She was attacked with erysipelas of the face and head on the 4th, which was removed a few days after, but she had no increase or relapse of the sore throat.

I now adopt the same plan of treatment in patients of all ages, with this exception, viz., that of applying leeches to the throat of children instead of bleeding at the arm.

Here, then, we have a remedy always at command, capable of removing the disease in 24 or 48 hours, unless suppuration have taken place before professional advice has been sought. Indeed, even if suppuration have already formed in the glands, nothing sets them at liberty sooner than cutting them and letting out the matter. The operation scarcely produces any pain, and the tonsils seem to me to have a remarkably quick tendency to heal up after it.

In conclusion, I may remark, that a kind of *phagedenic ulcerated sore throat* prevailed here in March and April last. It in general attacked children and young people, from the ages of two years up to twelve and fourteen, and spread in an extraordinarily rapid manner. It did not confine its ravages to the glands and fauces alone, for in one instance I observed it to attack the mucous membrane of the throat as well. It proved extremely fatal amongst the little ones. The remedial measures which proved most successful in my hands were, the warm-bath, antimonials in conjunction with calomel, and gargles with *acid. muriat.* I have no doubt but that many cases proved fatal from the want of gargles and other suitable local remedies, as every practitioner must be aware of the difficulty of inducing children to gargle their throats, or of applying anything locally to them.

I am, Sir, your obedient servant,

E. WILKINSON.

Aspatria, Cumberland,
August 11th, 1834.

EFFECTS PRODUCED ON NERVES BY THE

"MINERAL MAGNET."

To the Editor of THE LANCET.

SIR,—Knowing your readiness to promulgate whatever tends to the advancement of scientific inquiry, permit me to call the attention of your readers to a circumstance of great importance to medical science, which occurred to a patient of mine a short time ago.

A lady had been long troubled with an unsightly tumour, which was attached to the lower portion of the upper eyelid, near its external angle. She was likewise a martyr to neuralgia spasmodica facei (tic douloureux of the face) at the supra orbitar portion of the nerve of the fifth pair. For the relief of this painful malady she applied to me, to be placed under magnetic treatment; and I am happy to say that ever since the application of the magnetic fluid to the nerve, the tumour has gradually disappeared, and at present not the slightest vestige of its former existence remains.

Should any of your readers feel desirous to investigate this remarkable phenomenon, and would wish to have further

information on the subject, I shall be happy to furnish it, and would also give the address of one of the medical gentlemen who advised the lady to have the tumour removed with the knife.

I am, Sir, yours very respectfully,

E. S. BLUNDELL, M.D.

18, Lower Seymour-street,
Portman-Square, August 19th, 1834.

ALLEGED DEATH FROM TAKING

"MORISON'S PILLS."

——— Fungar vice cotis, acutum
Reddere quæ ferrum valet, exsors ipsa secandi.
HOR.

To the Editor of THE LANCET.

SIR,—In consequence of a very wretched statement which appeared in the Worcester papers of last week, purporting to be an account of an inquest held here on the body of a young girl who was killed by taking two of "*Morison's pills*," I consider that it is my duty to give rather a plainer account of the transaction than that which was poked into the corners of those two very respectable prints. If they had given a fair account of the matter, I should not have intruded myself on your notice, and I beg to apologize for so doing.

Rebecca Cross, æt. 15, was quite well on Thursday, July the 10th. Her mother imagined that she required some aperient medicine, sent her to purchase a box of "Morison's pills," gave her two at bed-time, and took two herself. The mother suffered great pain all night, but got better in the course of the following day. Her daughter was also in great pain all the night, as well as on the day after, during which time the pills operated with violence three times. The following night was one of great suffering to her. On Saturday morning I was called in, and found her labouring under severe enteritis. The most active treatment was immediately pursued. In consequence of my having formed a very unfavourable opinion of the case in the first instance, I requested my intelligent friend Mr. Woodward to see the patient with me, which he was kind enough to do, as well as to attend her in conjunction with my assistant, Mr. Robertson, while I was at Birmingham, at the annual meeting of the "Provincial Medical and Surgical Association." However, our united efforts proved unavailing; our patient died on the morning of the 30th, and I, in the presence of the before-named gentlemen, examined the body about thirty hours after death.

The deceased was emaciated to the last degree. The abdomen was enormously swollen, and on opening into the cavity of the peritoneum an immense quantity of gas escaped. The whole of the intestines were glued together, and a great proportion of their contents had escaped into the cavity of the peritoneum, the whole surface of which had been intensely inflamed. Between the stomach and liver there was a large quantity of purulent matter, which was confined by the adhesion of those viscera, and there was a cavity on the under side of the left lobe, large enough to contain an egg. At the head of the cæcum there was a hole, through which I passed my thumb without difficulty. Distant from that about six or eight inches, in the colon, there was one still larger, and the arch of that viscus was of a greenish-black colour, and could be torn with the greatest facility. The whole of the mucous membrane was studded with ulcers of various sizes, and, upon the whole, it presented such a mass of disease as I had never before seen.

Under these circumstances, I did consider it my duty to write to the coroner on the subject, who instantly summoned (through the proper officer) a jury, which met on the 31st. The jury consisted of *eighteen* individuals, who, with the exception of three or four, were highly respectable. Those whom I may perhaps except were individuals who had taken large quantities of the pills, and fancied they had derived great benefit therefrom. One, whom I may mention, was a labourer who was sent on purpose to occupy a place on the jury, and, consequently, was a very unfit person for the office. I may also mention that the leader of the party was the brother-in-law of the person who sells the pills. I gave it decidedly as my opinion that the pills were the cause of death. Mr. Woodward did the same. The majority of the jury did not require any further evidence, but as they could not agree as to their verdict, they were, necessarily, locked up for a considerable time. However, at last twelve agreed that the said Rebecca Cross died in consequence of taking "Morison's pills," and this must appear to any unprejudiced person to be an honest verdict. I am, Sir, your very obedient servant,

FRANCIS DAVIES.

Pershore, Aug. 13th, 1834.

P.S.—In reading over my friend Mr. Sheward's letter, I find there an allusion to some conversation that we had at my house at the latter end of June. I certainly then told him that I fully intended bringing the matter at issue before the Provincial Medical Association at Birming-

ham, but that from circumstances which occurred subsequently to our meeting I did not do so. It is my intention, however, to obtain the opinion of the Association next year at Oxford. It would be well if the matter was set at rest, as to whether it is legitimate and consistent that physicians should meet *sow-gelders* in consultation, and form an alliance with * * * *, thereby contributing to the utmost of their power to annihilate and destroy the regular practitioner * * * * "What advantageth it me" that I have been three years in London at a heavy expense studying my profession, if individuals, who have never attended a single lecture, are to be taken by the hand, sanctioned and *patronised* by our leading physicians?

Pecuniam in loco negligere
Interdum maximum est lucrum.—TER.

SUPPOSED

DECOMPOSITION OF SULPHUR.

To the Editor of THE LANCET.

SIR,—About a week ago I had put into my hands, by a friend (a medical gentleman), the three numbers of THE LANCET, containing Mr. Hiley's Essay on the Atomic Theory. That excellent essay I read with delight; but was not a little surprised at Mr. Hiley's remark, that "till very lately sulphur was considered by all chemists as a simple element;" for, like the generality of scientific chemists, I did not know that any satisfactory evidence existed that sulphur had yet been decomposed. He, however, goes on to say, "the experiments of Mr. J. M. Corbett, inserted in the 515th number of the *Mechanic's Magazine*, seem to prove that it ought not to rank among elementary substances any longer; for, according to the statements of that gentleman, he has effected its decomposition by means of lightning." Mr. Hiley then transcribes the following passage from the *Mechanic's Magazine*:—"I (Mr. Corbett) enclosed some sulphur in a glass tube, of two feet long by one inch in diameter. I passed a very fine spiral wire through the sulphur, and then fixed the whole in a metallic lightning conductor, which was insulated above the sulphur apparatus. The glass was so contrived, that any air coming from it would pass into a receiver placed for its reception. I now waited for the lightning to pass down the rod, and on visiting the spot, I found the spiral wire fused, and the lower part of the sulphur changed into a powder as white as snow, and my receiver full of hydrogen. I have named this new substance thiogen. Its specific gravity is 1.707."

I have referred to the *Mechanic's Magazine*, and found the original passage; by which I find that the apparatus was arranged two months before a shock of lightning passed down the conductor.

Now, by this passage, Mr. Corbett so very imperfectly describes the experiment by which he supposes sulphur to have been decomposed, that it appears to me highly premature to conclude from its result that sulphur is not a simple but a compound body.

Had Mr. Corbett given a very particular statement (which in a case bearing such an important aspect is most essentially requisite) of the manner in which he arranged his apparatus; of the quantity of sulphur put into the tube; of the quantity of sulphur changed into a white powder; of the quantity of hydrogen gas found in the receiver; and, above all, whether he effectually defended the sulphur, and the interior of his apparatus, from the acquisition of water during the two months he had to wait for the electrical discharge; we should have been better able to judge whether the hydrogen was actually disengaged from the sulphur, or it resulted from the decomposition of water which had accidentally got into the way of the electrical discharge.

Until assured that water was effectually shut out of the interior of that part of the apparatus through which the electrical discharge went, I cannot think that Mr. Corbett's statement proves sulphur to be a compound body. If water was not effectually shut out, he would undoubtedly find hydrogen in his receiver, from the decomposition of water: and, if the hydrogen resulted from the decomposition of water, there must have been some oxygen disengaged from that hydrogen. Would it not, therefore, be as fair to presume that the white powder which Mr. Corbett calls "*thiogen*" was a combination of oxygen and sulphur, perhaps the protoxide, as that it was a constituent part of sulphur?

I make these few remarks, in order that the notion of sulphur being a compound body may not rest too heavily on the minds of those who entertain it, until the matter is attested by further and unequivocal experiments. As 14 months have now elapsed since Mr. Corbett announced the matter, we may suppose that from experiments which, in the interim, he has probably been making, he will, ere long, either confirm the reality of the conclusion he drew from his first experiment, or say in what the cause of error laid.

If he prove his first conclusion to be right, I shall, with him, rejoice, as his discovery will open a wide field for further improvement in chemical science. I am, sir, yours, &c. H. H. WATSON.

Little Bolton, August 16, 1834.

RECORD

OF THE

CONCOURS HELD AT PARIS,

IN JUNE, JULY, AND AUGUST, 1834,

To supply the Vacancy in the Chair of

CLINICAL SURGERY,

OCCASIONED BY THE DEATH OF BARON BOYER.

(Continued from page 730.)

[THE following remark and reply of MM. BERARD and VELPEAU were designed to be added to the argumentation between those surgeons, on the thesis of M. Velpeau, with which our notice of last week was concluded at page 730.]

M. BERARD.—Amongst the symptoms which you give of sudden compression of the brain, as paralysis, stertorous respiration, paleness of visage, slow pulse, dilated pupil, &c., you have omitted one which is of much greater importance, and a more sure indication of compression than that last mentioned, viz., immobility of the pupil. Besides, you have neglected to distinguish recent depressions from those of old standing. Finally, in speaking of the several points of the skull on which the trepan may be applied, and the obstacles &c., you have not taken into account the prominent ridge running up from the crista galli, along the centre of the frontal bone.

M. VELPEAU.—In all diseases, but particularly those affecting the brain, the symptoms are variable; sometimes you find one predominant, sometimes another, or the same organic change may be accompanied by elevation or depression of sensibility. In compression of the brain I think we generally find the pupil dilated. Where it remains immoveable, there is another cause acting on the nervous system, viz. irritation. As to the distinction of old and recent depressions, I had not to enter into that question at all, my business being merely to study in general the cases requiring the trepan; besides, it is not the depression of itself which may require an operation, but the intensity of the compression produced by it. In reply to your last objection of my having omitted to notice the ridge of the frontal bone, I say we never have any indication for trepanning precisely over that line, because we never have effusion there; the fluid is always lodged a little on either side.

THESIS OF M. LISFRANC,

ON THE DIFFERENT METHODS EMPLOYED FOR THE OBLITERATION OF THE ARTERIES IN THE TREATMENT OF ANEURYSM — THEIR RESPECTIVE ADVANTAGES AND DISADVANTAGES.

Our labours as reporters and analysts of the proceedings under the concours now draw towards a close. We have arrived at the only thesis and argumentation yet to be noticed, in order to complete the agreeable but complex and not easy task which we undertook, as journalists, at the commencement of these chirurgical assemblies in the French capital. Our readers may be assured, that it has required no small degree of zeal and assiduity to note such debates as the present, conducted in a language foreign to an English ear, and held in a theatre which has been occupied, day after day, every day truly a "dog-day," by two thousand students and practitioners of medicine. We find it, however, impossible this week wholly to dismiss the subject of the concours, but it will, we believe, only remain for us, in another number, to give a summary of the "anterior titles" of the candidates, and that shall be done as soon as we can collect the necessary information.

The thesis now before us is that of M. LISFRANC. The subject of the discourse is indicated at the head of our remarks, and we are bound by our conviction to say that it is certainly equal, if not decidedly superior, in merit, to either of the other theses of the concours.

The order which the author has pursued, the extent of his researches, and the sound judgment exercised in seizing the important points of the subject, leave little to be desired. The work is divided into seven parts. In the first, M. Lisfranc passes in review the surgical anatomy of aneurysms, and endeavours to ascertain what is the comparative frequence of those tumours with respect to age, sex, and the

artery affected. He defines an aneurysm to be "a tumour formed by arterial blood, and communicating with an artery,"—and asks what parts most commonly concur in the formation of the sac. This question, according to the author, has not been sufficiently resolved, at any rate as regards the sac in cases of traumatic aneurysm. In spontaneous aneurysms, the first capital fact is their being almost exclusively confined to the aortic system. In more than 300 cases, collected from his own practice and from authors, M. Lisfranc has found only two (and those subject to objections) in which the pulmonary artery was affected. This remark gives rise to an anatomical description of the arterial tunics, and of some pathological conditions which explain the above fact.

M. Lisfranc describes as existing between the internal and fibrous coats of the large aortic arteries, a hard, dense, fragile coat (the "tunique sclereuse" of Dr. Malgaigne) which is the seat of the ossific concretions so often observed. At the same time he remarks, that there is no well-authenticated example of ossification in the pulmonary artery which does not possess this special tunic. (This latter assertion is however not correct. Hodgson, we remember well, describes a case of ossification of the pulmonary artery, of which we have ourselves seen an example.)

The observations made on a limited scale by Hodgson, to determine the influence of age and sex in the production of aneurysm, are extended and modified by M. Lisfranc. The former author collected 63 cases, of which 56 were in the male, and 7 in the female, or 8 to 1. M. Lisfranc has collected 154 cases; 141 for men, and 13 for women, or 11 to 1.

The results of Hodgson's investigations to determine the relative frequence of the disease with respect to the artery affected, were very incomplete. M. Lisfranc has examined 179 cases of spontaneous aneurysm (the arch excepted), and draws the following deductions:—popliteal artery, 59; crural artery at groin, 26; ditto lower down, 18; carotid, 17; subclavian, 16; axillary at axilla, 14; external iliac, 5; innominata, 4; humeral, 3; all the other vessels, 3, 2, and 1.

Finally, the author examines 101 observations, in which the patient's age was noted, and finds that aneurysms are most common between the ages of 30 and 50, while they are excessively rare before 20, or after 60 years of age.

The second chapter of the thesis contains the historical part, in which M. Lisfranc proposes to remove some errors of the best writers. This has occasioned a very warm discussion between him and M. Dèzeimèris, sub-librarian to the Faculty of Medicine, and author of a dictionary on the history of medicine.

M. Lisfranc attributes the first method for the treatment of aneurysm to Rufus; it is thus described by Actius:—"Si vas unde emanat sanguis profundum fuerit, ubi situm ejus et magnitudinem diligenter perspexeris, noverisque numquid vena sit an arteria, *vas immissa volsella extendemus, et moderate circumflectemus.* Ac ubi ne sic quidem cessaverit, vinculo constringemus; nonnunquam et post vinculi nexum oblique vas incidere cogimur."

The author regards this as applicable to aneurysm, and remarks that we find in the words "vas immissa," &c., the origin of torsion of the arteries. It appears to us, from a simple inspection of the words, that Actius speaks rather of hemorrhage from a wounded vessel; besides, it appears that the translation adopted by M. Lisfranc is a bad one, and not found in the best editions of Actius.

The origin of the method which in England we call "John Hunter's," has, for a long time, been a matter of controversy. In France, Hunter's method is called by the name of Anel. Several authors have carried up this method to Guillemeau, but M. Dèzeimèris proves that the latter merely followed Actius. Lisfranc, however, insists that Guillemeau has indicated Anel's method, which he traces up to Ambrose Paré, from whom he quotes the following passage as a proof:—"Portant, je conseille au jeune chirurgien qu'il se garde d'ouvrir les aneurismes, si elles ne sont fort petites, et en parties non dangereuses, coupant le cuir au dessus, le separant de l'artere: puis on passera une esguille à seton, enfillée d'un fort fil, par sous l'artere aux deux costés de la plaie, laissant tomber le filet de soi-même et se faisant nature engendre chair, qui sera cause de boucher l'artere."

For our own parts we cannot find in these words the method of Hunter and Anel, although M. Lisfranc professes to

do so. It is quite clear that Paré speaks only of large aneurysms in dangerous places (as the neck, groin, &c.), while his practice was to open small aneurysms in other parts of the body.

But enough of history. In the next chapter the author treats of the different methods of obliteration practised from the earliest times to the present day. These he distinguishes according as they have been applied immediately to the tumour, above or below it.

First.—*Methods applied to the Tumour.*

1st. *Styptics and refrigerants* were very early used. For the last forty years nothing but the treatment by refrigerants has been employed in the hospitals of Bordeaux, and experience shows it to be a sufficiently efficacious means.

2nd. *Compression.*—Since the time of Ambrose Paré, a variety of ways has been proposed to compress the sac. That of Guttani has had the greatest reputation, and he pretends to have cured four patients in fifteen. M. Lisfranc has seen a spontaneous aneurysm as large as the fist cured in six weeks by the application of metallic plates and compresses. In cases of traumatic aneurysm, M. Lisfranc says compression should never be applied immediately over the wound, but above it, for fear of determining inflammation and gangrene; besides, this method is only applicable to a few cases, where the artery is small and the effusion circumscribed.

3rd. *Incision and plugging.*—This method well deserves to be abandoned, from the danger of second hemorrhage, inflammation, abundant suppuration, gangrene, &c.

4th. *Suture of the arterial wound,* only applicable to traumatic aneurysms, was proposed by Lambert, who passed a needle through the wound of the vessel at the fold of the arm: the vessel was obliterated. M. Velpeau quotes Asmann as having produced obliteration of the arteries in animals by the same means.

5th. *Electro-puncture.*—M. Pravay has proposed to pass needles into the tumour, and guide a galvanic current on the blood. Scudamore has already shown how powerfully this coagulates the blood, but there are no facts to prove its utility.

6. *Cauterization.*—M. Larrey has often employed with success moxas, opposite the tumour of an internal aneurysm. Might not the same remedy be applied to external aneurysms? Experience must determine.

Second.—*Methods applied to the Artery, between the Tumour and Heart.*

1st. *Compression.*—The author puts *immediate* compression aside, as being only a variety of the ligature, and details the general rules to be observed in the employment of *mediate* compression. He then shows what circumstances favour the formation of the clot; in what parts of the body it may be tried with most advantage; and finally asks, how does compression act on the vessel? Scarpa and Jameson think, by giving rise to adhesive inflammation; but in a majority of cases M. Lisfranc thinks it acts by determining the formation of a clot. Hence he approves of M. Malgaigne's practice of placing another compress at the other side of the tumour. This method is much superior to that of compression exercised immediately over the tumour or wound in the vessel, and it is better, again, to compress all along the limb; by this latter method the successful cases exceed the failures, while compression of one point of the vessel above the aneurysm gives an average of five cures to thirteen.

2nd. *Ligature.*—In the immense field opened by this subject, the author proposes merely to examine methods which are sanctioned by experience, or those which require further investigation. He commences by a brief though perfect description of the method of applying the ligature in general, the precautions to be used, the means of avoiding or correcting errors, &c., then analyses the results obtained by Jones and Manec in their experiments, and gives the most complete account of Amussat's method that is perhaps anywhere to be found. The various substances employed to tie the vessel are also considered, and the experiments of Jameson made with animal substances are noticed. He certainly obtained a good deal of success, but Manec, who has extensively experimented on the subject, always found that the ligature was sooner or later discharged by abscess, and the experience of M. Dupuytren gives a similar result. Hence the author prefers the common ligature, cutting off one end close to the knot. MM. Physick and Levert, in America, have made various experiments on animals with metallic ligatures, and the latter describes the wound as constantly healing over the foreign body, which remained perfectly tranquil, and also produced obliteration of the vessel. It does not appear that this method has been tried on the human body, but the author considers this and other methods of the same kind as offering too little advantages to make them preferable to the simple one of the common ligature.

3rd. *Immediate Compression.*—The method of Scarpa, sufficiently well known, is that generally used; however, it is often

followed by accidents. M. Roux has made an extensive trial of this method; he has employed it about fifty times on the principal vessels of the neck and limbs, and only had five or six cases in which the patient was carried off by gangrene or hemorrhage.

4th. *Mechanical Plugs.*—These were used before the discovery of the ligature by Ambrose Paré, and have been recently employed also. Some introduce a piece of wax into the mouth of the vessel, and push it up. In cases of ossified arteries MM. Roux and Dupuytren introduce a morsel of bougie or other soft substance, and apply a ligature over all.

5th. *Seton.*—This was first tried by Jameson, of Baltimore, on sheep and dogs, who thus obtained obliteration of the carotid artery. His experiments were repeated in Paris, in 1829, with the same results. M. Amussat passed a thread by a needle through the vessel, and drew one end until the other became free within the vessel; a clot is formed round this foreign body in small vessels, and they are obliterated, but not so when the artery is of a certain caliber.

6th. *Acupuncture.*—M. Velpeau passed a needle through the femoral artery of a dog, and in five days the vessel was closed by a clot; after several experiments he found that for an artery as large as a common quill one needle was enough, while larger vessels required two, three, or more. But M. Amussat passed four needles through the carotid of a horse, who was killed after a lapse of sixty hours; there was no clot formed. This is, therefore, a doubtful method, and not to be attempted on the human subject.

7th. *Torsion.*—The history and manner of performing this operation are sufficiently well known. M. Lisfranc describes the latter in full, and details the phenomena accompanying it.

8th. *Refoulement.*—This also belongs to M. Amussat: to perform this operation, the end of the artery is drawn down a little and well separated from the surrounding tissues; it is then seized transversely with a small forceps (*pince a baguettes*), a similar forceps is applied just above this, and also transversely, but in the opposite direction; the internal and middle tunics are now divided by simple pressure of the forceps: this done, the forceps next the heart is kept fixed, and the other worked obliquely, until it descends on the vessel for some way, pushing before it the internal and middle coats; this is sufficient to arrest the circulation and cause the formation of a clot. This method is applicable to arteries in an undivided state; by pushing the coats on the side removed from the heart, there is more chance of a clot being formed. It has been tried only once on the human subject, and is abandoned by the author, though constantly successful on animals.

9th. *Contusion* (*des machures*).—Whenever an artery is pressed between the blades of a round-bladed forceps, the internal and middle coats are completely divided, but the external one remains intact. This is not sufficient of itself to produce a clot. M. Amussat, therefore, tried it, in conjunction with a ligature, *below* the contusions. Several divisions were made on the carotid of a large dog, and a ligature was applied; on killing the dog a clot was found, as is usual near the ligature, which sent in lateral prolongations, strongly adherent to the divided edges of the tunics. This experiment always succeeded; at the end of four days the adherences of the clot are so strong that the ligature may be removed. This operation has not as yet been tried on the human subject, but M. Lisfranc thinks that, unless something unforeseen occurs, it is calculated to create a revolution in the practice of surgical hemorrhage, and to supersede, in great measure, the ligature.

10th. *Brasdor's Method.*—The history of this operation and the success attending it, are sufficiently known to our readers; we therefore dispense with noticing the details into which M. Lisfranc enters.

From the preceding analysis it appears that there are three principal methods applicable to an aneurysmal artery; viz. those which act *on* the tumour, and those acting *above* and *below* it.

Those employed *on* the tumour, are in general little efficacious, some are dangerous; and others, as electro-puncture, not established by any experiments.

Those employed *above* the tumour are more sure; but of these many have been tried on animals only, and are too dangerous to be admitted in practice, as the seton, acupuncture, refoulement, &c.; three others, viz., mediate and immediate compression, and the ligature, are not exempt from dangers or inconveniences; finally, two others, though not yet tried on man, deserve to be employed, from the advantages they promise; these are torsion, and, principally, contusion (*machures*), aided by ligature.

Finally, Brasdor's method should never be tried except when the second process is impossible, and when compression, cold, &c., have failed.

Spontaneous Aneurysm admits of the application of all the various processes described, according to the case. M. Lis-

franc, however, rejects altogether the ancient method, and recommends, in preference, Paré's, otherwise called Anel's or Hunter's.

Traumatic Aneurysm requires some special principles, and it is still much debated amongst surgeons, where in these cases the ligature should be placed. Some tie both ends, and Mr. Guthrie insists chiefly on this practice. Dupuytren and Delpech are content with a single one. It would appear from a just estimation of the facts, that when the arterial lesion is recent one ligature is generally sufficient. When the sac is formed and hemorrhage has often taken place, the same practice succeeds, but exposes to the danger of secondary bleeding: M. Lisfranc applies two ligatures only when the vessel is superficially situated, or for an old wound of the vessel where the trajet has become fistulous; or, finally, when an artificial aneurysmal sac has opened and given rise to the bleeding.

We cannot terminate the present article without noticing a table attached to M. Lisfranc's thesis (our analysis of which we have now completed), which at once shows immense literary research, and may serve as a foundation for several curious and useful deductions.

The table contains 242 cases of aneurysm, and indicates, in parallel columns, the work in which the case is described; the surgeon treating; patient's name; age; artery affected, and species of aneurysm; process employed; accidents; date of the ligature coming away; and termination. It is subdivided into sections, according to the different methods employed.

I. II. and III. contain 18 cases which were treated by refrigerants and Valsalva's method. In 5 cases treated by Valsalva's process there are 4 cures (carotid and subclavian aneurysms); the remaining 13, treated by styptics, refrigerants, &c., give 6 failures, 6 cures, 1 uncertain.

IV. Compression between tumour and heart, 13 cases; 8 unsuccessful, 5 cured.

V. Compression on the tumour, 5 cases; 1 cure (aneurysm by anastomosis of temporal artery).

VI. Compression on the whole limb; 5 cases; 3 cures, 2 failures.

VII. Compression without indication of the spot, 24 cases; cures 11, failures 13.

VIII. Ligature after the old method, 31 cases; failures 6, cures 23, 2 doubtful.

IX. Anel's, or Hunter's, method, 151 cases; failures 45, cures 106.

X. Brasdor's method, 14 cases; 4 cures, 10 failures.

XI. Torsion—*refoulement*, 1 case; cured without accident.

ARGUMENTATIONS

ON THE FOREGOING

THESIS OF M. LISFRANC,

BY MM. BERARD, LEPELLETIER, AND BLANDIN.

Remarks of M. Berard, and Replies of M. Lisfranc.

M. Berard commenced by remarking that M. Lisfranc had attributed, in the historical part of his thesis, several omissions and errors to M. Dèzeimèris which were not really his. Thus, in page 14, he quotes Actius as translating Rufus thus: "Si vas unde emanet sanguis profun-" dum fuerit, ubi situm ejus et magnitudi-" nem diligenter perspexeris, noverisque " numquid vena sit an arteria, vas immissa " volsella extendemus, et moderate cir-" cumflectemus. Ac ubi ne sic quidem " cessaverit, vinculo constringemus: non-" nunquam et post vinculi nexum oblique " vas incidere cogimur."

You attribute, said M. Berard, this passage to Rufus, whereas it does not belong to him at all, as has been sufficiently proved. Besides, the passage does not refer, as you say, to an aneurysmal tumour, but to arterial or venous hemorrhage. This is the natural sense of the passage, and I do not see how it can be distorted so as to meet your interpretation.

M. Lisfranc.—Rufus here speaks of blood effused in the deep parts, or of traumatic aneurysm: each person is at liberty to understand the passage as he thinks best: you differ from me, but I adhere to the interpretation which I have given. As to the name of M. Dèzeimèris, which you have just mentioned, I am quite ignorant in what manner I have hurt the feelings of that gentleman. It is true that I have put forth in my thesis opinions somewhat different from his; but this is a circumstance both permitted, and even common, in all scientific discussions, and I never had the idea of personally attacking a writer for whose erudition and talents I have a high respect. It was therefore with the utmost surprise that I found M. Dèzeimèris had distributed in the court, two or three hours ago, a publication directed against my thesis: I hold one of them in my hand: and if you pro-

pose to enter into a discussion, founded on that writing, it is probable, considering the special studies of M. Dèzeimèris, that my chances of giving a satisfactory answer will be considerably diminished. I cannot help, however, remarking, that it would have been much more worthy of M. Dèzeimèris to have waited until the conclusion of the concourse, when I had no other adversaries: the medical journals would then have offered a much more proper channel of discussion, and he would have found me ready to meet him on that ground, as he shall find me if he makes the trial.—(*These few observations were received with the most enthusiastic cheering on the part of the pupils. Silence was presently restored.*)

M. Berard then assured the candidate in the chair, that any observations he would have made would not have been founded on the publication of M. Dèzeimèris, which he had not seen; but after what M. Lisfranc had said, he thought it his duty to abstain from referring to the historical part of the thesis.

The other candidates successively made the same declaration.

M. Berard.—In that part of the thesis where you speak of the different methods of treatment, I expected to find the treatment of the ancients examined, compared, and opposed to that of the moderns; but I do not find a word on the advantages or disadvantages of that process. This appears to me to be a grave omission.

M. Lisfranc.—I have not neglected to make mention of the process employed by the ancients, for I say they tied the vessel above and below the tumour, and made an incision into the latter. If you look to page 110, you will find these words:—"Spontaneous aneurysm admits of every species of treatment, according to the exigency of the case. We do not speak of the ancient method, as it is almost universally rejected at the present day," &c. You see, then, I do not neglect this process, but reject it altogether, as being essentially bad. In a thesis, where the subject is so large and extensive as mine, it is impossible to treat all the minor details, especially those concerning a mode of treatment which should never be employed. The ancient method is one of this kind; it has so many grave inconveniences attending it, that very few modern surgeons ever think of employing it. Boyer, in his great work on surgical diseases, has abandoned the operation; I could not, therefore, sacrifice time to the consideration of a process which, as I said before, was too imperfect and dangerous to be used.

M. Berard.—The subject of your thesis is "the different methods employed for the obliteration of the artery in cases of aneurysm; you should, therefore, have examined the ancient method, even supposing it to be a bad one, as you say; however, the operation is neither so bad nor so obsolete as you represent. It was performed not long ago at the Hospital of La Charité, and, in some cases, should even be preferred to the other processes.

M. Lisfranc.—I uphold that all modern authors reject this method as essentially bad, and that it was, therefore, sufficient to mention it, without entering into further details. I know that it has been employed by others, and I have done it myself; but the inconveniences are too great to induce me ever to repeat the experiment.

M. Berard.—When speaking of refrigerants applied to the tumour, you say, "We can conceive how the action of cold, propagated to the liquid blood contained in the aneurysm, may determine its coagulation, and the consequent obliteration of the tumour and artery." Now the experiments made on the blood by Hunter, Scudamore, and others, show that a low temperature, far from contributing to the coagulation of the blood, rather opposes it, and renders it slower. The application of ice and refrigerants cannot, therefore, have the results you describe.

M. Lisfranc.—The argument and deduction which I have employed, are founded on the simple practical fact, that when you place ice on an aneurysmal tumour for a length of time, the tumour becomes solid, without doubt from the coagulation of the blood contained in it. For forty years Guerin employed no other treatment in the hospital of Bourdeaux, and was tolerably successful. Besides, you should take into account the difference between experiments performed on blood out of the economy and blood contained in the sac of an aneurysm; they are not placed in circumstances strictly analogous, and this may explain any variety of effect. I do not say that blood is coagulated in a living vessel by cold, but merely in the aneurysmal sac, and this, I think, is confirmed by experience.

M. Berard.—The effect of extreme cold on the blood while contained in its vessels has also been tried, and is perfectly well known. In freezing the ears of an animal, it was easy to see whether the blood in the vessels was coagulated or not; and when the substance of the ear was divided, the blood was found to be in a fluid state. The cold had not, therefore, the effect of freezing the blood in the vessels.

M. Lisfranc.—I never said the ice was applied with the idea of freezing the blood contained in the sac, but simply for the purpose of promoting its coagulation. Every surgeon knows well enough the danger of gangrene, which might be produced by a degree of cold enough to freeze the blood. The application of ice to the tumour is a case quite different from that you quote; here the blood is in part coagulated, and already disposed to take on that process; it stagnates in part in the sac, and presents the same mass of substance to the refrigerant body; there is, in short, no comparison between them.

M. Berard.—I consider the analogy sufficient to support the reasoning I have employed; in both cases the blood is living, and contained in its proper vessels. You reject all the points of resemblance, and dwell only on the differences. I do not see, however, that you have answered the main point of my objection, viz., the impossibility of cold favouring the coagulation of the blood.

Remarks of M. Lepelletier, and Replies of M. Blandin.

M. Lepelletier, after a few observations, in which he abandoned altogether the discussion of the historical part of the thesis, in consequence of M. Dèzeimèris's publication, commenced by attacking M. Lisfranc's definition of aneurysm. "a tumour formed by arterial blood and communicating with an artery." This definition would comprehend several tumours not aneurysms; again, he thought M. Lisfranc should have taken care to distinguish from aneurysms, simple arterial wounds.

M. Lisfranc defended at length his definition, and showed how it was necessary to distinguish aneurysm from other tumours containing blood; he also proved that he had not neglected to describe and distinguish wounds of an artery from aneurysm.

M. Lepelletier then insisted on the objection of M. Berard as to the action of cold, which he developed at some length; he could not admit the explanation given by M. Lisfranc; besides, he regarded the application of ice as more dangerous than it could be useful, from the reaction it is apt to excite in the system.

M. Lisfranc repeated his former explanation, which he said was not so necessary, because whatever became of the explanation, whether it was good or bad, the fact of cold producing solidification of the tumour was no less certain; as to the danger of reaction he knew perfectly well that cold might produce this effect, and he had even mentioned a case of it at page 27; but when reaction of this kind did occur, it was easy to moderate it by sanguineous evacuations, &c.

M. Lepelletier was far from being satisfied with this explanation, more particularly as the author had spoken of moxas (page 35) which might "propagate their action deeply to the interior of the sac, determine there a salutary irritation, and cause the formation of clots." Now as the moxa could only act by its heat, he saw a manifest contradiction between the two ideas.

M. Lisfranc.—You have mistaken altogether the manner in which a moxa acts. It does not produce its effect by mere heat,—I never said that, nor did it ever enter my head,—but by irritation. I therefore ask whether this irritation may not be capable of producing contraction of the sac, and I think it is probable that it may.

M. Lepelletier, after some observations which turned more on the sense of passages than on any error of doctrine, remarked that the author enumerated among the processes tried only on animals "refoulement." Now this was incorrect; the operation had been tried on man.

M. Lisfranc acknowledged that an error had there glided in; but remarked that the only operation of the kind which had been performed (by M. Amussat) was noticed in the table at the end of the thesis.

M. Lepelletier returned to the attack on M. Lisfranc's exclusion of the ancient method of operating. He asked why he excluded it altogether at page 110, while at page 112 he says he prefers it; "for varicose aneurysm, compression on the tumour is applicable when it is small and recent; under the opposite circumstances Anel's method seldom produces advantageous results, and I prefer *opening the sac and applying a double ligature*." Besides, said M. Lepelletier, if we look to the table at the end of your thesis, we find the facts there cited more in favour of the ancient method which you condemn so strongly. The deaths are as one to four, while by Anel's method the deaths are as one to three, minus a fraction.

M. Lisfranc.—The difference between the results of the two methods is easily explained; by the new methods all kinds of aneurysms are treated, and the surgeon does not hesitate to apply it to the largest vessels; on the contrary, the method of the ancients was applied only to a few aneurysms, and those in themselves not very dangerous. They never, for example, operated on the aorta, on the iliac arteries, or on the brachio-cephalic. This will account for their comparative success with

an operation which I still hold to be essentially bad.

Remarks of M. Blandin, and Replies of M. Lisfranc.

M. BLANDIN prefaced his attack by some observations on his peculiar attachment to anatomy, which was the reason, said he, of attacking M. Lisfranc on that ground, in preference to any other. There were a few anatomical points in the thesis to which he could not assent. Thus, in describing the anatomical structure of the arteries, you say the middle coat is composed of spiral fibres. I deny that the fibres are disposed like the spring of a suspender; I have examined the structure of the arteries with great care, and have always found the fibres transverse, making only one turn round the vessel, so that you could separate them from one another without rupture.

M. LISFRANC.—There is a great difference between anatomists on this point. I know that some deny the spiral form, but most authors describe them as being a little oblique, and I agree in this opinion.

M. BLANDIN.—After having quoted several modern anatomists of high reputation who agree with him, observed that in page 38 he found another anatomical error. In speaking of compression on the brachial artery, you advise it to be employed as low down as possible, to avoid the compression of the radial nerve. Now I say there is not the slightest danger of compressing the radial nerve even in the middle of the arm; your observation is correct for the median nerve, but not at all so for the radial.

M. LISFRANC begged that words would not be put into his mouth which did not belong to him; he never spoke of the radial nerve, but of the orbital; he hoped he knew the position of the radial nerve as well as M. Blandin, and when he spoke of compression low down to avoid the nerve, he did not mean to say the nerve was there, but that by compressing *high* up, it might be injured.

After this followed a long discussion on the treatment of a wounded vessel.

M. LISFRANC says he would tie both ends, "when the artery is not very deep."

M. BLANDIN insisted that he should have added, "when I can tell exactly what vessel bleeds," for if the surgeon cannot precisely determine the vessel, he ought to tie the main trunk, and cited as a proof a wound of the neck.

M. LISFRANC then cited other cases in opposition, and attacked M. Blandin's practice in this case as bad.

As we have remarked, the concours will yet occupy a little more of our space.

THE LANCET.

London, Saturday, Aug. 23, 1834.

SOME of our contemporaries have stated, with becoming gravity, that the Chairman of the Medical Committee presented to the House of Commons, on Wednesday week, the report of the Committee, on the evidence which has been elicited relative to the state of medical education and practice in the British dominions. An extract taken from the Parliamentary record of a daily newspaper, inserted at page 735 of the last LANCET, may, from the construction of one of its sentences, have led to a similar inference. The exercise of a little reflection, however, must have convinced our readers, that it was not possible for the Committee to report on the *evidence* so early as Wednesday last,—the day, in fact, on which the sittings of the Committee were concluded. What actually passed in the House was this: The Chairman stated that the sittings of the Committee having been necessarily protracted until that day, the Committee had not been enabled to prepare their report on the evidence. The Committee having, then, recommended that the minutes of evidence still in manuscript should be printed, a motion to that effect was carried, *nem. con.* It is necessary that we should publish this explanation, in order, first, to relieve the minds of the profession from a suspicion that the Committee, after carrying through such a laborious inquiry, had formed a hasty, and, consequently, an imperfect report; and, secondly, to remove any impression that the House of Commons would wilfully withhold, for a single hour, any information which might be likely to afford satisfaction to such an influential body of men as the members of the medical profession.

When the importance of the inquiry is

sufficiently considered, and when the great object sought to be obtained by the investigation is kept in view, it must be regarded as a fortunate circumstance that the Chairman of the Committee will not be called on to furnish his report until some months have elapsed. The mass of evidence which that gentleman will have to scrutinize, is one of prodigious extent, occupying, possibly, not less than a thousand folio pages of print.

If the inferences which may be deduced from such an immense assemblage of facts are to be made with effect, it will be necessary to arrange the testimony of the witnesses into classes,—to draw comparisons with respect to the Universities,—to institute contrasts between the hospitals and the Universities, and between the capabilities of public and private schools,—to present a summary of the anomalies which arise from the present defective construction of medical statutes,—to expose, with a bold hand, the acts of injustice which have been perpetrated by the medical corporations,—and, having thus laid bare the deformities and corruptions of past and present systems, the Chairman, aided and supported by the Committee, will be required to recommend to the House and to the country the principles and details of a scheme of medical government which will prove equally conducive to the security of the public health, and the honour and protection of the members of the medical profession.

While Mr. WARBURTON is thus patriotically and benevolently labouring for the public welfare, the minds of the profession may in some measure be beneficially prepared for exercising a sound judgment on the report, by being made acquainted from time to time with some of the items furnished in the minutes of evidence taken before the Committee. We shall now, then, occasionally turn over the leaves of our reporter's note-book, and furnish the profession with the best means of forming an opinion as to the mode in which the inquiry has been conducted, and the extent to which it has been carried. Further, it may be advantageous as well as satisfactory to let them know, without delay, the unceremonious grinding to which certain Presidents and other official parties have been subjected. In the next LANCET we shall begin by exhibiting a few of the contortions of the eel-backed Baronet.

AT length we are compelled to abandon the silence we have hitherto maintained relative to the renewed outbreak of malignant cholera in this metropolis, and in various parts of the country.

When it was reported that the disease had made its appearance in London in the autumn of 1831, we ascertained by actual observation that the report had facts to sustain it, before we gave publicity to the terrifying announcement in the pages of this Journal. Having satisfied ourselves by an actual inspection of the sick, that the cholera of India, or a disease exactly resembling it in its effects and general symptoms, if not in its remote cause, had attacked a portion of the inhabitants of this metropolis, we hesitated not to warn the public of such an occurrence, and to urge the profession to make every effort to check the course of the distemper, and we admonished the Government to adopt those sanatory arrangements which experience and reason indicated as the best means of protecting health. At first we encountered no small portion of obloquy because we dared to publish the truth on the subject. It was pertinaciously denied that any new disease had made its appearance in London; and then it was contended that if a fatal disease, unknown in past times, had really broken out amongst us, it was *not* the cholera of

India. Unfortunately, however, the statements which we at first published, and from time to time repeated, received a melancholy confirmation in the fatal events of every day. When it was no longer possible to deny the existence of the disease,—when it was no longer possible, without the most unblushing effrontery, to allege that it was not that direful disease which had slain its hundreds of thousands in India, then was the public attention called to the question of contagion, and the ravages of the malady were, for a time, almost concealed from the public, by the veil which several heartless disputants endeavoured to throw over its supposed causes.

Without, at this time, resuming the subject of quarantine further than to state our firm belief, that no restrictions which may be imposed upon common intercourse, connected with trade, can prevent the spread of this dreadful malady, whose poison, there seems much reason to believe, floats in the air,—without again taking up the question regarding the contagiousness of cholera, or its communicability from man to man,—we find ourselves called upon, as journalists, to record the fact, that the malignant cholera has again broken out in London and different parts of the empire, and with a degree of virulence which was unprecedented in the two previous pestilential visitations. That the attacks are not so numerous as during the worst periods of 1832, is true; but it is remarked by all who have had an opportunity of noticing or treating the disease, that the violence of its onset has, unless opposed by a form of treatment to which we shall presently advert, been too terrific to admit of resistance by the powers of the human constitution. In the numerous cases which have fallen under the notice of private practitioners of our acquaintance, the disease, almost from the first moment, has assumed the worst possible form, and left no doubt on the mind of the beholder that the malady was identical with the malignant cholera of India. Virulent as is the pestilence in the heart of London, it has been remarked that it is not less so in the suburbs of the metropolis; and its fatal effects have been experienced in villages, ten and twelve miles from town, which have been long celebrated for their salubrity.

As heretofore, this frightful disease is no respecter of persons. It has attacked the high and the low, the rich and the poor, the overfed and the underfed, the infirm and the robust, the aged and the young. As it has alike visited the houses of the nobility, and penetrated the workhouses and jails, selecting its victims as well in the most airy, open, and apparently healthy situations, as in the darkest courts, alleys, and cellars; no sound or satisfactory inference can be drawn as to those circumstances which govern its movements or influence its visitations.

In this state of uncertainty and bewilderment relative to the primitive sources of the disease, we are bound to be governed in our conduct by those facts which we do understand, and which have already furnished to us ample means of admonition. Instead, therefore, of engaging in controversies which, from the existence of prejudices, can scarcely be guided to any beneficial result, we ought, as men of sense and prudence, to seize whatever advantage our experience of the disease has already furnished, and employ it unhesitatingly and boldly for the benefit of the afflicted.

Already, then, has it been proved that we ought to avoid at a time like this every occurrence which is likely to lead to a derangement of the health. But it is of especial importance that the most scrupulous care should be observed in the selection of our daily food,—that those substances should be avoided which are crude

and indigestible, which produce flatulence and uneasiness of the stomach, or gripings of the intestines. At the same time a careful abstinence should be maintained with respect to the drinking of fermented liquors. We notice this circumstance because it has been observed in many very many instances, that after a looseness of the bowels has existed for some days, cholera in its worst form has suddenly made its appearance, and carried off its victims in a few hours. Not to mention other instances it may be stated that a wholesale stationer of opulence and respectability, after having been the subject of an unheeded diarrhœa for two or three days, lately experienced an attack of cholera, and although he was sufficiently well to walk from his private residence in Buckingham-street, Adelphi, to his place of business in the city in the morning, he was a corpse in his counting-house in the afternoon. So dreadfully severe were the spasms, and so sudden the death, that time was not allowed to remove the patient to a bed, or even to take off his clothes. Had the premonitory symptoms which characterized the case of this gentleman, received that due share of attention which their importance demanded at a threatening epoch like this, he might at this moment have been alive and in good health, but until an hour or two of his death, he obstinately refused whatever assistance it was in the power of medical skill to afford. In other places four, five, and six persons, attacked in one dwelling, equally negligent or thoughtless, have delayed applying for medical advice, until, alas! medicine could be of no avail. At Camberwell, last week, in a single house, of five persons attacked, four expired within a very brief period; and the following passage, extracted from a letter which was published in *The Times* of Thursday, will show that in one dwelling in the densely-populated neighbourhood of Clare-market, within one hundred yards of this office, the disease has been equally violent and fatal. The letter is from Mr. F. Shoberl, Jun., of No. 4, Leicester-street, Leicester-square:—

"The following circumstances, to the accuracy of which I can bear witness, occurred within the last few days:—A woman, who lodged at the Old King's-arms, in Houghton-street, Clare-market, died. The disease was pronounced to be the true Asiatic cholera. Nevertheless, it excited no alarm in the house, and no precaution was taken to prevent its further influence. A woman who had some clothes to wash from the house was attacked soon after washing them. The next case was Mrs. Spraggs, the landlady, who, after eating a hearty supper, was seized, and died in the course of 48 hours. Soon after, a compositor, named Hulme, in my employ, who lodged in the house, was likewise attacked. All these cases occurred within a few hours of one another; all the patients were similarly affected, all had every symptom of the malignant blue cholera, and in all alike the disorder terminated fatally."

Here, again, four individuals perished in one house, and in the case of the washerwoman it certainly would appear that the disease had been communicated by contagion or infection. Let us, however, turn from this sad and gloomy picture to one of a more gratifying character.

We have seen that the disease is most violent in its attacks, most fatal in its effects. Experience has taught us, then, that we should avoid all the exciting causes of the malady, and, amongst others, none with a more rigid or prudential care than indigestible or unwholesome food, avoiding strong drinks as poison, and giving the preference, whenever there is the opportunity, to home-brewed beer, rather than to the acid compound called "porter," which is sold by the great brewers. In all things, at a crisis like this, frugality is enjoined. But should we be incapable, by precautionary measures, of warding off the disease, what is then to be done? On this point—viz. that of the treatment of cholera, we refer with

feelings of infinite satisfaction to the communication of Mr. BEAMAN, inserted at page 754 of this week's LANCET. The concurrent testimony of all practitioners with whom we have conversed on the subject, the evidence of many authors of eminence who have written on malignant cholera, stand recorded in favour of treating the disease by emetics. Accordingly, many practitioners, observing the decidedly good effects which have resulted either from the ejection of noxious matters from the stomach, or from the powerful efforts made by the stomach and muscles to discharge such matters, have laboured with praiseworthy zeal to discover what medicine would act the most speedily as an emetic in attacks of cholera. Various have been the remedies recommended with this view, and, amongst others, one of the earliest that was employed, was the common *culinary salt* in a state of solution. An incidental reference to the utility of this remedy was made at the *London Medical Society* when the disease first appeared in this country. It was adverted to on various occasions afterwards, but from some cause it appears to have fallen into disuse, or only to have been rendered a valuable remedial agent in the hands of a few practitioners. Of late again we have heard it mentioned in terms of the highest commendation, and these circumstances combined tend to stamp the communication of Mr. BEAMAN as one of the most important that has been made connected with the treatment of malignant cholera. To those of our readers who are unacquainted with the professional character of Mr. BEAMAN it may be necessary to state, that he is a scientific practitioner, a lover of his profession, and a most honourable man,—a gentleman, in fact, on whose testimony the most implicit confidence may be reposed. If there were no other evidence but that of this eminent surgeon, relative to the efficacy of *salt emetics* in the worst,—observe, the worst—cases of cholera, we should be content on his recommendation alone to announce it as a remedy deserving of a general trial by the members of the faculty; but as we read and hear that it has also been successful in the hands of other practitioners, thus giving an ample confirmation of the testimony of Mr. BEAMAN, we take this the first satisfactory opportunity of advising that it should be employed in every case where it appears to be demanded. Let it be given as prescribed by Mr. BEAMAN; let it be administered in the worst cases, for while there is life there is hope. The object, be it remembered, is to produce energetic vomiting as quickly as possible. Hence the dose of the solution should be repeated at brief intervals, in order that that object may be accomplished.

In making trial of this remedy, we must entreat our professional brethren to note down the results of their practice, and furnish us with the particulars at the earliest opportunity. We do not ask for long detailed cases, which would require much time in the concoction. A communication of the leading facts will be sufficient to answer every good public purpose, and we do hope and believe that there is no legally-qualified medical practitioner in the empire who will conceal from the profession what may be the results of this or any other remedy that may be employed by him in the treatment of such a dreadful disease as the malignant cholera.

THE EDINBURGH WITNESSES.

(*From our Edinburgh Correspondent.*)

WE have been all kept alive here on the subject of "medical reform" since the return of the *witnesses*, and it is quite evident, from all I can see and learn, that they consider, to use a homely expression, that the "game is up." Perhaps you are not

aware that but few of these witnesses were *summoned* by the Committee. Drs. ABERCROMBY and CHRISTISON were the only *paid* men; Drs. M'LAGAN, THOMSON jun., and BALLINGALL, went as *amateurs*, or, rather, as emissaries, to *support* the monopolists and corruptionists. As I remarked to you before, nothing of any consequence was elicited from the crafty ABERCROMBIE. Dr. CHRISTISON defended, powerfully, the indefensible part of the University, but Dr. ALISON was too honest and single-minded to make any stand in favour of corruption, and the "liberals" had the satisfaction of seeing him *break down*.

Dr. BRIGGS, of St. Andrew's, is understood to have made a strong impression in favour of the rights and privileges of that University, and candidly stated that he and his professional brethren there had never been interfered with or molested by the Medical Faculty of the Edinburgh University, as long as they sold degrees *cheap*, and could produce some good "certificates" of London manufacture; but whenever the price was advanced at all, and an *examination* of the candidates was required, then came forward the Edinburgh junto, conceiving their personal interests to be in danger, and endeavoured to throw odium on the St. Andrew's degree.

Dr. ROBERTSON, one of our private teachers, and one of the examiners of the St. Andrew's faculty, disgorged some important information. He detailed, in particular, the whole system of chair-making in the Edinburgh University, which was first laid bare to public view in the pages of your Journal, and when the undisputed talents of Dr. JOHN THOMSON, in intrigue and chair-making, were denounced to the Committee, Hogarth would have found in JOHN'S son's countenance, I am told, ample occupation for his pencil.

Dr. SHANKEY'S appearance was made as that of a *volunteer*, and the corrupt object of his mission rendered his examination truly contemptible. Dr. POOLE, the son of our old hotel-keeper, was sent up to wait upon Mr. WARBURTON, and give him an account of the College of Physicians; he is said to have given no intelligible or direct answers, and, so far, dexterously performed the duties of his metropolitan visit. Enough, however, was squeezed out of the learned doctor to show that the College of Physicians *here* is even at a lower ebb than is the venerable institution in Pall Mall East, and that scarcely any persons belong to it but the professors of the University, and the accoucheurs, *one and all*, of auld Reekie.

REFUSAL OF A PHYSICIAN OF GUY'S HOSPITAL TO VISIT A SICK PUPIL, SINCE DECEASED FROM CHOLERA.

To the Editor of THE LANCET.

SIR,—Your valuable pages having always been open for the exposition of abuses of all kinds relating to the medical profession, I trust that the observations which I am about to make may not be deemed unworthy of a small space in your ensuing Number.

On Sunday afternoon, Mr. Tanner, a pupil of *Guy's Hospital*, was seized with incipient symptoms of cholera. Two gentlemen, *pupils* of the Hospital, remained with him at his request throughout the evening, during which time he became gradually worse. At about 11 o'clock he expressed a wish that Dr. Back's* advice should be obtained. Accordingly, one of the gentlemen immediately proceeded to the Doctor's house, which was certainly not more than five minutes' walk from the residence of the patient. He sent up a message by a servant to the following effect, namely,—that a pupil of Guy's Hospital was taken ill with cholera, and wished to obtain his advice. The gentleman heard this message delivered by the servant to the Dr. when what was his surprise to receive in answer to his message the following courteous reply from the Doctor:—"That he (Dr. B.) would not come, as he made it a rule never to visit after dark!" The gentleman went back and stated the Doctor's reply to Mr. Tanner, who, after a short time had elapsed, expressed a desire that Dr. Bright should be sent for. The other gentleman therefore immediately proceeded to the west end of the town, where the residence of the Doctor is situated, who, the moment he heard of the case, came down with all possible celerity. Notwithstanding all his efforts, however, the unfortunate young man expired at 1 o'clock on the ensuing morning (Monday).

* Dr. BACK is one of the physicians of Guy's Hospital.—ED. L.

Such are, I am certain, the facts which occurred, and such being the case, in what manner can we regard the conduct of Dr. Back upon this occasion? But I shall leave the readers of this letter to form their own opinions, as I have not been induced to send these remarks to you for publication for the sake of traducing Dr. Back's character, but in the hope that by their appearance before the whole profession the Doctor will be induced to give some satisfactory reasons for his conduct. I think perhaps, Sir, that a few observations upon this subject from your able pen will produce a greater effect in the proper quarter than the more humble remarks of your correspondent who signs himself,

A FRIEND TO PHILANTHROPY.

Southwark, August 6th, 1834.

[The publication of this letter was postponed until it had received sufficient authentication.—ED. L.]

A DISPENSARY CASE!

To the Editor of THE LANCET.

SIR,—In the *Medical Gazette* of June the 12th, which I happened to receive only yesterday, there appears a report of a case which is, by the author, termed "strangulated hernia insidiously fatal;" valuable for the ingenious imbecility with which he condemns his irresolution in not having proceeded to an operation for the relief of his patient. I may be allowed to ask the writer whether there do not appear in his relation of the case, other circumstances, other omissions, more worthy of condemnation than the irresolution with which he charges himself, an irresolution of which he, no doubt, is determined never again to be guilty. The writer appears to have been informed by the patient that his rupture had been of six or seven years' standing; and he informs us that, satisfied as to its nature, he endeavoured to reduce it; but that, after making considerable pressure for about a quarter of an hour, he found not the least diminution of its bulk. He adds also, there was no distention of the abdomen, but pressure on this part (whether on the abdomen or on the hernia is uncertain) caused some slight degree of pain. The writer goes on to state that the patient had been at the *Islington Dispensary*, and had been seen by the resident medical officer, by whom he was informed that after his (the medical officer) having made pressure on the tumour for about half an hour, he had reduced the greater part of the hernia; but that there still remained something in the sac; that the patient expressed no pain from the attempts made at reduction, and that there were, at that time (24 hours before he was dead) no symptoms which could induce him (the medical resident officer) to suspect strangulation. It appears further on in this detail, that the patient told the writer that, two days before he had received a blow, either on the abdomen or hernia, the hernia had from that time caused him some uneasiness, and had increased in size, &c.

This patient the writer of the article directed to take a small dose of castor oil and tincture of senna! and in case of vomiting or pain coming on, to send immediately to the Dispensary. This remedy appears to have been taken at three o'clock p.m., and to have been repeated at eight o'clock, as the bowels had not been acted upon by the first dose. Here ends the treatment! The sequel is almost too distressing for reflection; the poor man fell the victim, not of the irresolution of the surgeon, in his not having proceeded to an operation, but to his not having resorted to more efficient means for obviating the results which were to be naturally expected to arise from the blow inflicted on his abdomen, and the very injudicious and empirical pressures and kneadings to which he was subjected.

Good God, sir, can the professional knowledge and experience of medical gentlemen who are selected for the important duties of dispensaries and infirmaries in this great capital be, at the present day, so low, so defective, as to be productive of such fatal errors as to lead a medical man to be satisfied with the administration of a little castor oil and tincture of senna as the only remedial measure resorted to in a case so important,—as to lead to no solicitude as to the advancement and progress of a case wherein the conviction of the surgeon's mind was, that he had to contend with strangulated hernia,—as to suffer him to neglect all inquiry about the result of the absurd and inefficient means resorted to for relief? Though impressed with this conviction, still he does not appear to have anticipated the probability, I should rather have said, the certainty, of inflammatory action ensuing; because, forsooth, the circulation was not discovered by him to have been disturbed. He neither bleeds his patient, nor applies leeches to or about the tumour, nor to the parts of the abdomen which he states to be tender from the effects of the blow, nor does he attempt to excite the intestinal action by glyster, nor to soothe the sufferings of his patient by the warm-bath, nor to relax the system by small and moderately nauseating doses

of tartar emetic, nor by opiates, but he does the very worst of all injudicious things, he takes means to excite the parts of the intestinal canal superior to the protruded portion of the intestine, by the administration of purgative medicines, which by the excitement of an effort to expel their contents, could not fail in aggravating the sufferings of his patient.

This case is too distressing any longer to engage attention; but before I abandon the subject, I may be allowed to recommend the officers of the *Islington Dispensary* to read with more care,—more accurately to ascertain the condition of parts concerned in old herniæ, in which they will meet with considerable condensation and thickening, but too well calculated to deceive the young and inexperienced practitioner, often leading him to believe that although he has returned the greater portion of the protruded mass, still some remains are left in the sac, whilst neither sac nor protruded mass exists. If they will thus consider and reflect they will cease to insult medical readers with their jargon about the internal and external abdominal rings, and about those old hernias extending obliquely downwards from one to the other aperture, for in old hernias every man who has been engaged in practice, and who, as an accurate anatomist, knows how to avail himself of the opportunities of post-mortem examinations, will discover that those superior and inferior, or internal and external, orifices, so evident in recent hernias are, in old ones, so altered as to become almost direct or perpendicular, and that although in the latter, as well as in the former, the elevations from the protruded intestines, &c. &c., pass obliquely downwards, still it is obvious that our manipulation in our efforts at reduction must, under those different circumstances, be widely different; for the recent hernia ranges through an oblique muscular canal; the old hernia escapes through a round aperture, and projects itself towards the groin and scrotum, covered only by the skin and the cellular tissue, but the latter, as has been before observed, often infiltrated, is often thickened, and condensed.

I am, sir, your very obedient, servant,

F. L. Nicholas, M.D.

A Retired Peripatetic, &c.

London, Aug. 12, 1834.

THE ARMY MEDICAL INCUBUS.

To the Editor of The Lancet.

Sir,—It is some small satisfaction to those who have been connected with the medical department of the Army, and who still retain an interest for the character of men who may have served in it, that your independent periodical should have laid before his civil professional brethren so just a portrait of their Director-General, for which many obligations are due to you. Without it he might have continued his imposing pretensions, not only to scientific acquirements, but as a promoter and promulgator of the sciences, in which he is little better than a charlatan, and in regard to his erudition and classical lore, we suspect he is equally scantily stored, as we do not recollect that his ambition has yet induced him, at any of his departmental *revels*, to pronounce an oration in any of the dead or foreign languages. We have repeatedly heard him in his vernacular tongue, when, poor man, he failed in putting three sentences correctly together. No, Sir, self! all-engrossing self is his strong hold, and has been the prop and main-spring of every action of his life. Self-aggrandisement, it is said, overpowers even the principle of T * * * *.

How a body of men whose education has given them respectability, and the power of making a just and reasonable remonstrance, could have remained so long silent under a system, introduced by him, which is repugnant to the feelings of every Englishman, it is difficult to say, —a system of espionage under the head of "Confidential Reports" *alias official whispers*, which is as dangerous to the individuals against whom they were levelled, as they are disreputable to the introducer; leading to partiality, injustice, and in some instances downright oppression; creating jealousy, disgust, and discontent in the department, unexampled in any other branch of the service,—facts not altogether concealed from the Horse Guards, where with profound wisdom they adhere to a principle of office, as unjust as it is unphilosophical, denoting the little mechanical minds from whence it emanates. Without investigating the real cause, they attribute the system to the peculiar education of those who compose the main body, and support the existing power, thereby increasing the evil by adding authority to error and injustice, and sinking in the scale of respectability and importance, a branch of the service weak in itself from its constituent parts, but of vital importance to the organization of an army.

The present chief, brought from obscurity by perseverance and some hard work, with not a little *booing* and low-cunning, and placed in an office of a conspicuous and respectable kind, has constantly sought to support and further his elevation by the means which succeeded at his commencement, but, like the pitcher

taken often to the well, his crooked ways have become so proverbial that, notwithstanding his baronetcy, he has sunk into that state of insignificance which those who longest and best know him always predicted would sooner or later be his fate; but it is deplorable to think that with him he has dragged the department, if not into the gulf of ruin at least to its brink.

It always has been a paradoxical question to those who laboured in the same field, how a person of his caliber succeeded with an officer of the Duke of Wellington's discrimination. Perhaps it is an extraordinary instance, how a little well-timed zeal and industry, with a good deal of the *Scotch accompaniment*, will obscure the discernment of the most acute.

As an old servant it would perhaps be painful to many to hear of his being placed in reduced pecuniary circumstances, especially as he now has a *baronet's family* to support, to do which he is known to have made a great, but it is supposed an unsuccessful, effort by saddling the department with his son as an agent. However, having succeeded in getting himself made civilian, which he long concealed from the department, there are few who would not rejoice at hearing of his enjoying the *otium cum dignitate* at Camden Hill, in order that his situation might be filled by some one more worthy of the enthusiastic support and many acts of friendship which have been from time to time bestowed, *ill bestowed*, upon him. It is impossible (vain as he may be) that he can be blind to the position in which he stands, and with him the department, and it must be superfluous to tell him, that with such a head the body can never regain what it has lost of late years.

It well becomes him to make a show of philanthropy by urging contributions from those who are at his mercy, for the purpose of glossing over his own acts of partiality in establishing funds for professedly charitable purposes, in many instances to be distributed amongst those who, under a strict system of justice and impartiality, in all probability would not now have to bewail in penury the loss of husbands and fathers of whom they have been bereft by the effects of climate whither they had been forced, while others of the *favoured clan* have been permitted to pursue their services unmolested, in conjunction with private practice in the United Kingdom, or have had other lucrative and salubrious stations. We envy him not his midnight reflections.

ONE OF THE SUFFERERS.

Dublin, July, 1834.

TUNBRIDGE WELLS DISPENSARY.

LETTER FROM MR. WAY.

To the Editor of THE LANCET.

SIR,—As you have admitted into the pages of your Journal a statement relative to the election of House-Surgeon to the above-named institution quite at variance with truth, which if uncontradicted might be prejudicial to the interests of the Dispensary as well as to the character of its medical officers, I am sure you will not hesitate to give insertion to the few facts, which, as being personally alluded to, I feel called upon to offer to yourself and the public on the subject. My object being only to correct error, I shall avoid any other notice of Mr. Cheyne's conduct in this instance but that of an expression of regret, that so respectable a gentleman should, from any cause, have been led to misrepresent a very plain and simple fact. Mr. Cheyne has professed an "unwillingness to colour his own case too highly," yet seems to have forgotten to mention that the "gratuitous performance of the duties of the situation" on which he founds a prior claim to the appointment, was for the accommodation and at the request of the then house-surgeon; neither has he informed you that his testimonials, high, very high as they undoubtedly were, still were *surpassed* in the opinion of many by those of another *unsuccessful* candidate.

Mr. Cheyne next remarks, that "my assistant had been applied to, and had consented not to practise at or within five miles of this place," and, "that such a measure was not sanctioned by any law of the Institution." In each of these particulars Mr. Cheyne is incorrect. I am authorized to say that *no application* was made or consented to, and also that such a regulation was adopted at the formation of the Dispensary (in consideration, but without the interference, of the medical officers), but that it has *never* been acted upon or observed.

A simple statement of facts will answer the last of Mr. Cheyne's charges, "that unfair advantage was taken by myself and others to bias certain members of the Committee." It was only thirty-six hours before the advertisement appeared in your LANCET, that my assistant communicated his wishes to me or to any one; feeling desirous to forward his interests, I wrote to one of the physicians on the subject, who recommended that no other application should be made till the vacancy had been publicly announced by you; this advice was strictly followed, and other

recommendatory letters, which I felt quite at liberty to write, were faithfully withheld till the Saturday, neither do I believe that a single *vote* was really solicited before that day.

On the day of election, five gentlemen having been declared eligible by the Committee, there voted for my assistant nine, for Mr. Cheyne six, and for a third candidate one, and I should inform you that the gentleman who was considered by many to have had superior testimonials, even to Mr. Cheyne, retired from the contest. You will not fail, Sir, to observe that *all* the candidates were considered *eligible*, and I may therefore say, without much fear of contradiction, that the motive which influenced the whole of the gentlemen who supported my assistant was to ensure as far as possible a lengthened residence amongst us by the appointment of an experienced *married man*.

Having made this explanation, I cannot but think, Sir, that the Committee will be acquitted of what Mr. Cheyne has called "jobbing" and "hole-and-corner work;" and as it is by no means my wish to engage in this kind of warfare, should he require any further information, he must expect it at other hands than mine. I have the honour to be, Sir, your obedient servant,

W. Way.

Tunbridge Wells, August 14, 1834.

FORCEPS FOR EXTRACTING DENTAL STUMPS.

We have received two letters from Mr. Shepherd, in reply to the remarks of Mr. Paul and Mr. Hunter on the subject of his dental stump forceps. Want of space has prevented us from giving an earlier insertion to his remarks, and we are now, also, unable to find room for any portions of them but those which bear directly on points calculated to interest our readers. In the first of his two letters Mr. Shepherd thus expresses himself:—

"It seems to me to be the intention of Mr. Paul to imply that Mr. Weiss has *not* my sanction to make the forceps for extracting stumps; justice to Mr. Weiss, therefore, requires that I should make the following statement:—

"When first I made the design for my forceps, Mr. Weiss was so much occupied that he requested me to postpone having the pattern made till he was more at leisure, but finding such an instrument as I had in contemplation indispensably necessary to my safe removal of stumps, I employed this Mr. Paul to execute it. He did so, and *did more;* he chose to dispose of some before the patterns were completed; however, by an effort on my part, I at last succeeded in obtaining about six or seven pairs, *all* differing from the original, and, like the Jew's razors, made to sell, each instrument having broken on the first or second time of using it. I stated in my account published in your Number of May 24th, that it is scarcely possible for my forceps to break a hollow stump; I was not then aware that the *hollow stump might break the forceps* (at least Mr. Paul's forceps) unless the material be good and well tempered. Thus then, as Mr. Adam Paul could neither furnish me with correct patterns nor good steel, I had sufficient cause to return to Mr. Weiss, of whose instruments I cannot speak too highly, having now tried them in many very difficult cases without the least accident whatever; and I cannot too strongly urge all who are disposed to adopt these forceps, to procure them of Mr. Weiss.

"Whilst on the subject, I may state that Mr. Weiss not only has the original pattern from me, but he is now in possession of eleven more, suitable to various cases, all of which I have found necessary: amongst these are two pairs of cutting forceps, for the purpose of separating the fangs of molar teeth when they are very firm in the socket, and are suspected to diverge, so that their extraction entire would endanger the jaw-bone. By this method the possibility of such an accident is entirely done away with. This practice, which is new, I frequently adopt in difficult cases, finding it much safer and less painful to remove the fangs separately, than to employ the force otherwise necessary. So very successfully have I found these forceps to act since my first introducing them to the notice of the profession, that I have carried on the same principle still farther, and now remove whole teeth in the same manner as I should remove a stump.

"Mr. Weiss is preparing patterns of these instruments also, from my designs, under the name of 'Safety Forceps,' their construction being such that it is next to impossible to break a tooth with them with ordinary pressure. Like the stump forceps they save much inconvenience by acting as a gum lancet at the time of their application around the tooth; they reach below the neck, and consequently below the usual seat of decay.

"With all the proposed improvements of late years, it is surprising that similar instruments both to these and the stump forceps, have not ere this been constructed. The latter I am informed, however, have been used *many years* by one of the most skilful operators of the present day; but

when a request to compare his instruments with mine remains unnoticed, and his dexterity with the elevator would imply an almost daily use of *that* instrument, I cannot imagine such to be the case, nor can I suppose any professional man so wanting in liberality as to have withheld for many years an instrument so safe for the patient, and so generally approved of as it has now been by the profession."

In corroboration of part of the above statement, Mr. Shepherd has forwarded to us a letter addressed to him by Mr. Giraud, of Faversham, dated May 26th, in which Mr. G., after speaking highly in praise of Mr. Shepherd's improvements, complains that on a second trial of the pair sent to him, he found "that one of the blades had begun to split at the extremity." In the next case, just as he had "grasped the tooth, the blade gave way entirely." He remarks that "this accident is the fault of the instrument-maker, Mr. Paul."

In his second letter Mr. Shepherd says:—"Feeling called upon to reply to some observations by a Mr. Hunter contained in your Number of this day, may I request the favour of you to give the following drawings a place in your LANCET at the same time with my letter of July the 16th, leaving it, as Mr. Hunter proposes, 'to the common sense of any reader to determine which forceps are best,' mine, the blades of which approximate at the points and grasp the stump equally from the base to the apex, or Mr. Hunter's, which can only touch at *one* point. The smoothness of the inner surface of the blades, which Mr. Hunter regards as a defect, *I* consider enables them to pass readily down between the stump and the socket.

"'The essential points of difference,' therefore, between my forceps and his are,—*the approximation of the blades, their internal smoothness*, and *cutting edges, none* of which 'essential points' *were* to be found in the 'precisely similar instrument' shown me by Mr. Lemale. In proof of the truth of this assertion, I may refer to Mr. Weiss, who, having compared Mr. Hunter's *pliers* with my forceps, will have great pleasure in receiving a visit from 'any gentleman who feels a doubt on the subject.'

"53, Baker Street, Portman Square."

*** We have this week received a letter from Mr. Thomas Gill, agent for patents, of No. 125, Strand, on the subject of this controversy, in which Mr. Gill speaks of it as "an attempt made by others to claim an invention of which Mr. Cyrus Fay was the real author, and for which Mr. Fay was rewarded with the large silver medal of the Society in the Adelphi. in 1826." Mr. Gill adds, "Mr. Fay has deposited two of his original instruments in my hands, in order that the profession may have the satisfaction of inspecting them."

No. 1.

Mr. Shepherd's Forceps, the blades of which approximate at the points.

No. 2.

Mr. Hunter's Forceps, resembling common pliers, the blades of which diverge.

LONDON HOSPITAL.

DISLOCATION OF THE FEMUR INTO THE FORAMEN OVALE.

JAMES TICHBERRY, aged 32, a gravedigger from Bow, was admitted into the London Hospital, on Tuesday afternoon, Aug. 19, under the care of Mr. ANDREWS, stating that, in stooping to stop a cricket-ball the day before, he slipt and fell on his side with his legs widely separated, and came to the hospital in consequence of the accident. On placing him upon the bed, it was found that he had an inguinal rupture on the right side, which was easily reduced. Attention was afterwards drawn to the right thigh, there being a total inability of approximating it to the other. Upon placing him in the upright posture, it was at once evident that the head of the femur was thrown into the foramen ovale. The body was bent forwards, the knees were widely separated, and the right extremity was lengthened about an inch and a half; there was a protuberance behind the pectinalis and adductor brevis, and the projection of the trochanter major was lost. He was taken up to the theatre and laid upon the table; a girth was passed

round the pelvis, and fixed on the opposite side to that of the dislocation, extension being made upwards and outwards, in the direction of the acetabulum, the foot being drawn as much as possible to the opposite side. As he was a very muscular man he was bled to ℥xxx, and took a grain of tartar emetic every ten minutes, but without producing nausea. The extension was kept up for half an hour without moving the head of the bone. He was then taken from the table, and placed in a sitting posture with a post between his thighs, extension being made in a line with the axis of the thigh, which, after a short time, was drawn forcibly inwards against the post, the extension being continued. In about five minutes the head of the bone slipt into its socket with an audible snap. JAMES A. WILLS, L. H.

ST. GEORGE'S HOSPITAL.

UNUNITED FRACTURE OF THE TIBIA AND FIBULA.

ON Thursday, July 17th, Mr. BRODIE performed a surgical operation, which will be better understood by our prefacing it with the following remarks. George Green, a little boy, had been admitted a few days previously, under Mr. BRODIE'S care, with an ununited fracture of the tibia and fibula of the right lower extremity. The accident, we believe, was received about a twelvemonth previously. The situation of the fracture was about midway between the extremities of the shaft of each bone, and as far as external examination and manipulation would show, was directly transverse. The broken extremities of the bones could be in some measure flexed on each other, and semi-rotated likewise, and from the extreme mobility of the parts, led Mr. BRODIE to conclude that an artificial joint had been formed. The manipulation of the parts gave the boy no pain whatever. As far as we could learn from a slight outline of the case read by the clinical clerk, it appears that at first after the receipt of the accident no care was paid to it, and it was neglected. The child was subsequently placed under the care of Mr. GRAINGER, who inserted a seton between the broken extremities of the bones, but without any effect. After this Mr. AMESBURY tried his plan of pressure, but without any better success, and the child was then sent to St. George's Hospital, under Mr. BRODIE'S care, who having on his first visit to the child heard the above particulars, and carefully examined the case, remarked, that it was one that demanded some consideration, and that he should reflect on it, and give his opinion on the case at his next visit. Accordingly, when next the boy was seen Mr. BRODIE said he had determined on cutting down upon the seat of fracture, and scraping away the cartilage or other substance with which the fractured ends were tipped, and dress the wound to the bottom with lint, with a view to excite inflammation, suppuration, and the pouring out of adhesive coagulable lymph, and thus to promote re-union of the fractured ends of the bones. Accordingly the following day the child was brought into the operating-theatre, and Mr. BRODIE made a crucial incision over the fractured end of the tibia *only*, and scraped away the soft cartilage that covered the fractured extremities of the bone, and dressed the wound to the bottom with lint. The operation lasted for a few minutes, and calls for no particular remark of praise or censure. The boy bore it like a little hero. Mr. BRODIE then addressed the following remarks to the pupils. All the surgeons left the room during the exordium, with the exception of Mr. KEATE, who kept his eyes reverently closed the whole time.

"I shall address a few remarks to you, Gentlemen, on the operation which you have just seen performed. I gave you a clinical lecture on the subject of ununited fracture last autumn, and for that reason shall not say so much to you on the subject now. This boy has ununited fracture of the tibia and fibula; he was under the care of a very clever surgeon in this town,* who placed a seton between the fractured ends of the bones; this plan was first adopted by Dr. PHYSICK of Philadelphia, but the successful cases, in which this plan was followed, that he published, were all fractures of the upper extremity, the humerus more particularly. I have tried this method in some instances, but have not found it successful in cases of fracture of the lower extremity. Mr. AMESBURY also had the child under his care, and tried his plan of pressing the two broken extremities of the bones forcibly together, with a view of exciting inflammation, and the formation of a bony callus, but this was also unsuccessful. There was a case of ununited fracture in this hospital, which was placed under Mr. AMESBURY'S own immediate care and superintendence, and therefore it may be said that the case was allowed the fairest chance of success in every respect, but it did not

* Mr. Grainger, of the Borough, we believe.

succeed.† Then this operation, which you have seen me perform to-day, I have performed before; sometimes it has succeeded, I was resolved therefore to try it again in this case. From the extreme mobility of the parts before the operation, I thought that there might be an artificial joint (for artificial joints will sometimes form in these cases, with cartilages, synovial membrane, synovial fluid, and synovial capsule; and in a volume of the Transactions of the Medical and Chirurgical Society, instituted by Dr. FORDYCE and Mr. HUNTER, there is an account of a very interesting dissection of such a joint by Mr. HUNTER), but I did not find such to be the case; the bones were tipped with a substance half ligament and half cartilage, for nature does not always possess sufficient power to throw out a perfect substance in these cases. You may, perhaps, ask me with a great deal of justice why I did not operate on the fibula (that bone being likewise broken) as well as the tibia. The tibia, you know, is covered by nothing beyond common integument, and, therefore, is more easily got at,—the fibula is a more difficult bone to reach, and does not bear near so much of the weight of the body as the tibia; it is placed rather as a sort of splint there, and serves for the attachment of the interosseous ligament and certain muscles. Considering, therefore, this bone as only secondary, I did not operate on it, for had I done so it would have made the operation a more dangerous one, and there would have been afterwards a large mass for suppuration among the muscles, which would have materially interfered with the future curative process of the case. The wound, as you see, was dressed to the bottom with lint, to promote inflammation, and this will be continued for some days, until the inflammation is sufficiently established, and pressure will then be applied to promote the osseous union of the fractured extremities of the bones."

July 21. The wound was dressed again to the bottom with lint and simple dressing; splints and bandages were reapplied. The boy looks remarkably well, and expressed a hope that he should not be taken again up there (to the operating theatre) to be scraped.

† As far as our memory serves us, there was a case of ununited fracture of the fore-arm in the old hospital under the care of Mr. Amesbury, but his plan did not succeed, in the new hospital, in Oxford Ward; the same plan was successful in a case of ununited fracture of the tibia in a coachman.

EMETICS IN CHOLERA.

IPECACUANHA I have seen of much use, and, from first to last, the use of *salt emetics* has been strongly recommended, many of the continental physicians, Becker of Berlin and others, thus commencing their treatment. Emetics, then, are of use in cholera; but in what cases and at what periods it becomes matter of importance to decide. Thirty cases are related in a letter of Sir D. Barry to have occurred in the practice of two German physicians, Drs. Ysenbeck and Brailow (who always, however, begin by bleeding) without the loss of one; and high commendation has been bestowed upon this plan by Mr. Neilson, Mr. Burton, and others in India; and Mr. Corbyn observes that the natives are in the habit of giving a strong solution of *common salt in water*, or the following mixture; one pice weight of onion juice, with twice its weight of arrack, both which have violent emetic effects, and it is said that they are very successful with this treatment. The benefit of an emetic was marked in the following case, which occurred under my observation.

Case.—Ann Homer, æt. 44, short in stature, plethoric, habitually short-breathed, and, as I have been since informed, notoriously intemperate, had been engaged to act as nurse at a cholera hospital. She had been taken to her bed incapacitated for her duty by drink. Such was her condition on the evening of the 7th of September.

Next day at five in the morning, she was suddenly attacked with vomiting, quickly succeeded by diarrhœa. The fluid discharges from the bowels were ejected with force, and continued many minutes at a time; in the intervals between the urgent calls to evacuate the bowels, a constant draining in the bed took place; some laudanum was given by the other nurse, but which failed to check in any degree the urgency of the symptoms. I first saw her four hours and a half after the commencement of the disease. Her countenance was then shrunk, her features sharpened, her lips and hands were livid; great anxiety was expressed, and she was troubled with insatiable thirst. She was immediately bled, though not to a very large amount, as her pulse was weak and did not rise. The blood when drawn had an inky appearance. She was again visited in three hours, and the only amendment that then was perceptible was, that the draining from the bowels had ceased at once after the bleeding, and that she had had but two evacuations

since. An emetic of salt and water was then exhibited, its full operation relieved the symptoms, the countenance became less livid, and perspiration took place. Some cramp, however, still remained in the extremities; purgative remedies were next employed, and all the primary symptoms ceased, a low fever resulted, and slight delirium preceded the appearance of the eruption which has been observed in some few instances in this country and occasionally abroad, after which her return to health was slow but progressive. This person's mode of life, and her condition preceding the attack, induced a decidedly unfavourable prognosis, which the character of the symptoms showed to be of the most malignant type.

Here then a malignant case was checked, and the disease was arrested by venesection, so far at least as the diarrhœa was concerned. Further measures were still requisite, and the obvious advantage of the emetic fully corroborated what had been urged in favour of this means of rousing the system, and putting a stop to the further progress of the disorder.

Those cases, I may add, in which vomiting is the chief symptom and is very urgent, may be brought successfully to a close, by keeping up the action of the stomach with copious draughts of cold water, to which class of cases that simple mode of treatment seems the most applicable. I had one case under my care which terminated favourably, in which nothing else was taken in the earlier period of the disease.—*Dr. Roupell's Croonian Lectures, Coll. of Phys.* 1833; p. 51.

For the introduction of Mustard Emetics we are indebted to Dr. Smith, of Newcastle. He had tried it in his own person, when resident in the West Indies, and, being aware of its being used as a popular remedy by the pitmen in cases of asphyxia from choke-damp, he was led to suppose that it might be of service in the collapsed stage of cholera. His suggestion was acted upon at Sunderland, through Dr. Gibson, with beneficial results, and has since been used to a considerable extent both in that place and at Newcastle. In the cold, blue, pulseless stage of the intense type of the disease, I believe it to be a very valuable remedy in relieving the irritation of the stomach, and exciting reaction, but when full vomiting can be exhibited by milder means, especially when it can be done by copious draughts of warm water only, I consider it safer to avoid the irritating effects of the mustard. —*Greenhow on the Cholera in Newcastle and Gateshead*, 1832, p. 78.

Having been informed by the surgeon to Sir John Malcolm, that some of the medical men with the Madras and Bombay troops in Malwa had used *emetics* and *antimonials* in many cases successfully, six cases were treated in the 26th Regt. at Mhow on their plan, after everything else had failed in all the preceding cases. The whole six recovered speedily, tartar emetic being employed, and whenever an opportunity occurs, I shall certainly have recourse to this mode of treatment. The successful termination warrants a farther trial.—*Dr. J. Thomson*, (26th Regt.), 1820, *in Dr. Hutchinson's Observations on Cholera Asphyxia*, 1832, p. 121.

Western Dispensary.—(*From a Correspondent.*)—The election of a successor to Mr. Clendinning, the late surgeon to this institution, took place on Friday the 8th inst., Mr. Malyn having obtained a majority of one hundred votes on the poll. The numbers were, for Mr. Malyn, 221; for Mr. Dobson, 121. Mr. Malyn is an able and indefatigable teacher of anatomy at the theatre of the late Joshua Brookes, and was an active coadjutor of Mr. Sadler and Lord Ashley, in bringing the condition of the factory children under the consideration of the committee of the House of Commons. Mr. Malyn having possessed (as physicians' assistant at the Manchester Infirmary for three years) extensive opportunities of observing the injurious effects of the factory system, his evidence on the subject was of the most valuable kind.

Bad Rice.—Cholera.—Dr. Tytler has handed to us, and wishes us to publish, the following extract from the "Commercial Report of the *Bengal Hurkaru*, of March 10, 1834, just received in this country, as a caution to those who are accustomed to use rice in England as an article of food:—"*Rice.*—The demand continues active for the Mauritius, and purchases have also been made *for shipments to England.* The price of Moonghy rice has considerably risen. Quality mixed and *inferior.*"

There are circumstances which render it very strange that a ballet should now be performing at *Kensington*, entitled, "The Princess and the Physician." The subject matter of the ballet has, doubtless, no reference to parties now living, but the selection of the title is very curious.

Mr. Brodie, the *Times* says, is to be made a baronet, and the title we think might be worse bestowed. He is a respectable man, though not a highly-gifted surgeon. If titles are still to be regarded as marks of honour by the public, there will be, we trust, many Brodies on whom the rank of Baronet will be conferred. It is interesting, however, to observe how the current runs in these matters. Sir Astley Cooper, with all his renown, could not succeed in obtaining the title of Baronet from a Tory government, until one of his brothers became a Tory member of the House of Commons. Mr. Brodie jogs on, and obtains no title; but presently the Whigs get into power, he attends the Whig Chancellor, a Whig brother is returned member for Salisbury, and then the little gentleman is dubbed Baronet. If there had been no Whig brother to give Whig votes, and no Whig Chancellor to attend, the sound of "Sir Benjamin," "Sir Benjamin," would never have tickled the ear of our aspiring little thunderer.

As that arch impostor John Long has ceased to live, a worthy votary of his residing at Camden-town has advertised that he is in possession of Long's "secret." If the nobility should nibble at a bait like this, they are lost past redemption. It is our duty to tell this simple advertiser that the "secret" of every knave is *dishonesty*.

CORRESPONDENTS.

H. B. It is not worth while to notice such a trumpery affair.

Medico-Chirurgus. If the surgeons of the *Swansea Infirmary* could procure the signature of a house-surgeon to such a bond, the penalty named would not be recoverable in a court of law. The judges have several times decided that such instruments are bad in law.

K.—A. B.—Mr. J. Harkness, and others. Advertisements should be sent to the Publisher, under cover to the Editor, postage paid, with a reference for payment for their insertion in town.

The "Query" on the subject of ringworm could not have been forwarded by our correspondent. It certainly was not enclosed with the other paper. Mr. Lloyd's reasons for not curing the disease are, doubtless, satisfactory to himself, but it is quite certain that they cannot satisfy the profession. If more attention be not paid to the diet of the children, it is impossible that they can be free from local or constitutional complaints.

W. B. B. If the facts can be proved in a court of law, as they are stated in the letter of our correspondent, the *chances* are strongly in favour of his recovering the amount of his bill; but if the steward himself recognised the employment of the surgeon in the name of the master, recovery is certain.

M. R. C. S. L., Brighton. Such vile practices will, without doubt, be put down under an improved form of law.

R. F. The charge for attendance on the medical and surgical practice of the *London University Hospital*, is 20 guineas. The hospital is announced to be opened for the reception of pupils and patients in October next.

H. R. The inscription of "Chemist" would not be a protection against a visitation from the Hall; that is, if our correspondent is to attend patients at their own houses, prescribe for them, and supply the medicines in medical cases.

E. Dr. Blundell *has* left *Guy's Hospital*, and, consequently, the Borough is without a lecturer on Midwifery, whom can we recommend. Of Mr. Ashwell's capabilities we know nothing; but "the published lectures" of young Ramsbotham of the Webb-street School, enable us to state that he is a silly twaddler. What wag induced that misplaced young man to believe that he could, *ex cathedra*, discharge the duties of *teacher*?

H. B.—Such a clause did exist in the bill; the arguments in its favour, consequently, are at present out of date; we, therefore, withhold the letter. Other points argued by our correspondent have re eatedly been made the subject of remark in our pages. His case was a very hard one, and one of those of which, we doubt not, another bill will tend to prevent the recurrence.

Notice.—It is particularly requested that all letters of business forwarded to the Office of The Lancet,—whether they relate to the contents of the journal, or refer to transactions connected with the Commission Department,—be invariably addressed to the Editor—Mr. Wakley. If this rule be not observed, it is impossible to guard against the impositions of anonymous scribblers who appear to have no other employment or pleasure than that of subjecting the conductors of journals to a heavy extortion in the shape of post-tax. If all letters of business transmitted to the office be addressed to the Editor, and no letters, unless so addressed, be received except the postage be paid, impositions will be effectually prevented.

Erratum.—In leader, last week, page 734, for "commands," *read* demands.